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THE ARTS.

VOL. XXVII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson, Thomas Forster, Esq.; J. A. De Luc, Esq. F. R S; Mr. Thomas Shute; Thomas Knight, Esq; Dr. Baird, Physician-General to the Navy; G. Cumberland, Esq.; Mr. P. Barlow, of the Royal Military Academy, Woolwich; John Wingfield, Esq.; Mr. John Cuthbertson; Marshall Hall, Esq. F. R. M. S. E.; Mr. F. Kerby, Mr Merrick, jun.; William Moore, Esq. of the Royal Military Academy, Woolwich; Mr. T. Noot; Thomas Thomson, M. D. Lecturer on Chemistry at Edinburgh, &c.

Of Foreign Works, Honoré Flaugergues; Mr. d'Arcet; Mr. Sonnini; Mr. Laplace; Count de Vargas; Prof Klaproth; Mr. Tromsdorff; Mr. L. Cordier, Mr. John; Mr. Haüy; Mr. Guyton-Morveau; Mr. Bouillon-Lagrange; Mr. Vogel; Mr. J. E. Berard; Mr. Gillet Laumont; Mr. Bucholz; Mr. Hersart; Prof. Henry Braconnot.

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The Engravings consist of 1. Figures illustrating the Growth of Seeds, in those of the Peach, Chestnut, Grass, and Palms, delineated after Nature in various Periods of their Evolution, by Mrs. A. Ibbetson; 2. Mr. De Luc's Electric Column, and Aerial Electroscope, 3. Mr. Shute's Scarificator on a new Principle; 4. Diagrams to illustrate the Theory of Capillary Attraction, by Thomas Knight, Esq. 5. Figures to illustrate the Structure and Classification of Seeds, delineated from Nature, by Mrs. A Ibbetson; 6. A Machine for raising large Stones out of the Earth, by Mr. Richardson; 7. Apparatus for Experiments on the Sonorous Properties of Gasses, by Mr. F. Kerby and Mr. Merrick, jun.; 8. Crystals of Apophyllite, or Fisheye Stone, by Mr. Haüy; 9. A new Hygrometer for Gasses, by Mr. Guyton-Morveau; 10 Delineations of Mr Congreve's Military Rockets; 11. The Cancer-Fulgens, discovered by the Right Hon. Sir Joseph Banks, of the natural Size; 12. The same Animal magnified; 13 The Medusa Pellucens, also found by Sir Joseph Banks, represented of the natural Magnitude.

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SEPTEMBER, 1810.

ARTICLE I.

On the Structure and Growth of Seeds. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

The author's inducement to write on the subject.

HAVING been requested by a gentleman, highly esteemed in the botanical world for the knowledge he has displayed in that science, to review the formation of seeds in general; to give a clear and concise picture of the growth of the embryo plant, from the first of its appearance in the seed vessel, to its shooting a perfect plant from the earth; to endeavour to prove the mistakes the variety of appellations have caused, as well as the misconceptions its extreme minuteness naturally occasions; and to show also in what order the several parts appear, as physiologists have differed much in this respect honoured by such a request, I shall venture to begin my task, trusting in the great magnifying powers of the various excellent instruments we now possess, and apologizing for venturing to contradict authors so much superior to me in science, as in this matter the eyes are the principal judges, and a long habit of viewing diminutive objects will detect truth, sooner than much knowledge. As I have before written on this subject, it will not be wholly in my power to avoid repetitions; but they shall be as few as possible.

Cause of the mistakes of physiologists.

It appears to me, that the manner in which seeds are generally taken for dissection is the cause of most of the mistakes disseminated. Most physiologists begin by dissecting full grown seeds; and, following their first impressions, applying the names, and appropriating the descriptions, to such parts of the seeds (in that state of growth) as appears best to suit them. But as there is not in nature any thing more different, than a very *young* seed, and an *old* one; it is ten to one that the names are misapplied, as the vessels of most consequence in the earliest stages are wholly lost in an old seed, while another set of vessels usurp (*almost*) their places; by which means the nourishing vessels, at the termination of their career, are often mistaken for the impregnating duct; while the vessel of life (before it reaches the heart) is taken for the *radicle*. So easily are these mistakes to be traced in most works of this kind, that I could give innumerable examples to prove my assertion, were it not better to show the origin of the mistake, and endeavour to rectify it; proving plainly, that to begin by dissecting full grown seeds is truly commencing at the wrong end.

Young seeds & old ones differ essentially.

Numerous instances of mistakes from this source might be adduced.

The seed should be examined in its earliest stage, and pursued through its whole progress.

To gain a perfect knowledge of the seed, it is necessary to detect its first appearance in the seed vessel; in this situation become perfectly acquainted with the names and uses of each part; and thence trace them upwards in their daily improvement, till, removed from the seed, they are fixed in the earth. To distinguish the vessels designed for the seed only from those intended to complete the *fruit*, is of the *first* consequence: for, as the seed is the principal *object* of Nature's care, it is the first perfected in its *vessels*; but, after they are all ready for the completion of the embryo, it takes a long time to prepare the young plant for its first appearance; and this time is employed by Nature in the completion of the fruit. This has, in most instances, two or three different vessels, not in any manner useful to the seed, found at this time invigorating the fruit; and as these mix much with the vessels of the seed, if the latter are not previously well known, they are but too apt to produce a very great confusion.

Commencement of life in the seed.

When the bud of the flower first appears, it is a notice Nature gives, that the seed has received *life*. It is then time to delineate the seed vessel, when we can best distinguish all the vessels of consequence, and perceive from which part they come, and for what office they are ordained. They are of three sorts. First, the impregnating, or vessel of life. This is always found in the middle of the seed vessel, proceeds directly from between the wood and pith in the stem of the tree, and becomes only the impregnating duct, when after entering the pistil it mounts to the stigma, and there receives the flower of the stamen, to mix with its own juices. Mirbel says, "The vessels of the style unite in the placenta, with those of the peduncle, and compose with them the umbilical cord." This is certainly a mistake: for the style is merely a *sheath* for the vessel of life, which only becomes the *impregnating* or *umbilical* vessel, when it returns from the stigma, conveying to the corculum of each seed the *joint juices*. This string I have often taken out at its length, after boiling away the pulpy part, and I could never perceive it was joined to any other.

Three sorts of vessels in it.
Vessel of life.

Mistake of Mirbel.

Nourishing vessels.

The second is a set of vessels called the nourishing vessels. These have not been long discovered, and I believe I was the first botanist that announced them. They run from the bark and inner bark, and bring not only support to the embryo, but the resins, oil, &c. for the formation of the young leaves. They enter the seed in bundles, and are by degrees disseminated all over it.

Vessel containing hidrogen.

The third vessel is a single one, and contains the hidrogen. It proceeds from the exterior of the plant, and enters at the back of the corculum or heart of the seed; but not till the formation is almost complete. It may indeed be said to give it the last finishing touch.

Three periods in the life of the embryo.

There are also three periods in the life of the embryo plant, concluding with its no lon[illegible]eriting this name, which, for want of being properly di[illegible]inated, have caused the greatest confusion among botanists. The first is from the time the seed appears in its seed vessel to its impregnation;

tion: the second from its impregnation to its readiness for planting: and the third from its being placed in the earth, till it rises thence as a growing plant. As I have taken many hundred seeds in this progressive manner, I could show their almost daily increase in regular drawings; but this would take far too much room. I shall however give in description as much as possible, adding a drawing of three or four of the intermediate times, to make it perfectly intelligible. For this I shall fix on the peach seed, the manner of growing of which is most generally applicable to all; the horse chestnut; and one of the grasses; to show any curious irregularity in the stated laws usually admitted. So very much do the interior of seeds resemble in their general features, that these three may serve as an epitome of all; for as in every seed there are eight parts common to all, it may well be supposed in so diminutive an object how great must be the general resemblance.

Eight different parts in every seed.

The eight parts are as follows: 1st, the line of life, or impregnating duct; 2d, the nourishing vessels; 3d, the corculum; 4th, the inner skin or pocket; 5th, the cuticle, or outward skin of the kernel; 6th, the stem of the embryo; 7th, the radicle; 8th, the cotyledons. I shall now describe the different stages of seeds, and confine myself to those circumstances, that are to be found almost *universally applicable*; and should I be thought prolix (which it is hardly possible to avoid on such a subject) I hope I shall be pardoned, if I am perfectly intelligible. I shall begin with a seed vessel of the chestnut cut both ways, as it is absolutely necessary to show the manner in which the different vessels may be traced in the seed vessels. Plate I, fig. 1, shows the seed vessel cut horizontally. It marks the distance at which different vessels enter. It is very curious to observe how it varies in place, till confined within the groove of the future stalk. Fig. 2 is the seed vessel cut lengthwise, to show how important a figure the vessel of life makes, when the seed is so young, and before impregnation: for at a later date it is hardly to be discerned, the pabulum of the seed having so increased in thickness, as to cover and inclose it. Though in both these there appear the rudiments of many seeds, yet it rarely happens, that there are more than one

Seed vessel of the horse chestnut.

Many seeds never perfected, but prepare juices for the others.

or two perfected; while the accumulation of juices prepared, and even formed into albumen, for the others, appears again to be dissolved into liquid nourishment for the favoured embryo. A beautiful provision of Nature: for, if those that did not arrive at maturity were to decay, so very much is the rot in vegetables liable to spread, few probably would perfect their seeds; whereas of the number that grow most of them serve as a reservoir of juices for the use of the most healthy of those the seed vessel contains. It is not difficult by long habit (when there are few) to discover their number by the protuberance of the outward cuticle: yet after this they disappear, and leave little or no vestige *but* a skin, *if that.* This is mostly however to be found in the seed vessels of trees, the glandiferes, nuciferes, &c., where the juices are easily communicated from one to the other.

In all seeds there never is but one embryo to be found in one kernel. The vessels in both halves should be traced with care to their origin, the impregnating vessel to its interior, the nourishing vessels to the exterior of the seed vessel, which suggests (what is afterward proved), that the latter receives much addition to its juices from the *dews* and *vapours* around.

The seeds receive part of their nutriment from without.

Part mistaken for the radicle.

I must now mention what no one has before noticed, but which is, I am persuaded, in part, the cause of the mistakes respecting the radicle, and the time of its appearing to grow in the seed. When the seed is taken in the seed vessel it shows a string (see *a*, *a*, *a*, *a*, figs. 1 and 2) running often, before it reaches the corculum of the seed, nearly round, and at a distance from the outward cover. Now this string, I am persuaded, has been taken for the radicle; or how could any botanist describe the radicle as preceding the cotyledons, when often *two months intervene* between them, the latter always shooting first? I will be bold to say, that the radicle of no seed ever did grow, till the *last epoch,* or its return into the earth. This string, which is the impregnating vessel, cannot enter into the seed, except at the corculum: it must therefore *stretch in length,* till it reaches this part, as the seed is not yet fixed, but swims and moves in a clear fluid. It may be observed in the plate, that scarcely *one* of the *seeds* given is turned the *same way*; but when the seeds leave the capsule, the vessels are confined within the stalk; besides, the business of the impregnating vessel has then been long over, and the string is either lost in the seed vessel, or melted away into nourishment for the rest; for it no longer appears: though I have my doubts, whether it is not the string found afterward passing over the kernel.

Periods in the growth of the seed. Commencement of the 1st period.

I now turn to the different stages of the growth of the seed. Figs. 3, 4, and 5, will display the kernel in the earliest stage of its first period. The pocket, *d*, is then filled with a transparent jelly. The corculum, *c*, is perfectly *empty*, being only distended by the parts adjoining. The impregnating vessel, *a*, joins the corculum to the pocket, which is still seen at some distance. Beside these vessels of *consequence*, there are generally found in the first stage some bringing air, others juices, to the new plant. I never before, except in water plants, discovered a real *air* vessel *in a vegetable*; but in the seeds they are certainly found, and it is almost impossible to conceive what a quantity these *little vessels yield*, and how continually the change of vessels is seen; each day brings a new assortment.

Air vessels in seeds.

End of the 1st period.

In the latter part of this period the pocket has greatly increased, and of course shortened the distance between the two bags; the nourishing vessels have entered the outward cover of the seed; and, instead of the air vessels before seen, a number of vessels running from the green part appear to assist in increasing the pocket. And here ends the first period.

Beginning of the 2d period.

Mistake of Mirbel.

The second begins with the impregnation of the embryo; and the first sign of it is, that the corculum is immediately filled with a glutinous liquor. Mirbel says, "Some time before and after impregnation, no change takes place in the interior of the plant." I have so long studied this part, that I must contradict him. I never knew the second noon pass, if the plant was affected, without making the impregnation visible; though I have certainly seen the stamen flower, without any consequence taking place with respect to the seed; but then the seeds *never germinate*, and if there was no visible effect, I cannot conceive how it could be known, that impregnation *had taken place.* On the contrary, the alertness of Nature is such, that often in 48 hours the corculum is filled, the impregnating line runs through the heart, and the increase of the nourishing vessels is visible. The line stops when the cotyledons shoot, but they do not yet appear.

2d stage of the 2d period

In the second stage of this period the spreading of the nourishing vessels is astonishing; the pocket and corculum join; and the cotyledons begin to grow. All this time no radicle appears, though the cotyledons have almost completed their form. It is in this state of advancement, that the seed declares whether it is a *seed leaf,* or *common seed:* for these two forms divide almost the whole assemblage of seeds.

Two divisions of seeds

Last stage of the 2d period. Cotyledons.

The conclusion of the second period shows the cotyledons in their natural form: that is, either with two little silvery thin leaves, perfectly white; or with two thick *yellow leaves,* which afterward, rising with the plant, turn green, and are seen above the earth. I have never seen more than two, except in the fir tribe and some of the grasses; and in the cress and mustard seed, the former of which has six cotyledons, and the latter four. This perhaps accounts for their springing so quickly out of the earth. With respect to the monocotyledons and dicotyledons I shall say a few words toward the conclusion of my letter, as I am perfectly convinced with Wildenouw, that the division is erroneous. This is the time for proving, that no radicle is yet to be seen. Where is the radicle in these seeds ready for planting, and prepared for it? see figs. 15—21.

Radicle.

When the radicle begins to shoot (and its work is soon done), the primordial leaves also show themselves between the cotyledons, see figs. 22—25: and it is in this stage, that the holders begin to grow, where there are such parts in the seeds; for few plants, in comparison of the innumerable species, *have them.* In some of the diadelphian tribe indeed they grow sooner, particularly in the beans. At the termination of the second period too is seen the use of that part in the seed, which is formed with peculiar strength, and not only marked where the vessels enter, but which a double cuticle covers; either in a *circular* spot, as in the *chesnut;*

chestnut; or in a long *cylindrical slip*, as in the pea. So violent is the force used against it, that, if it was not by various means greatly *strengthened*, the embryo would burst from the seed, long before its time, probably destroy itself by a premature birth, and tear the vessels in a manner highly inimical to their future *usefulness*. How beautiful the provisions of Nature! What care, what attention, to each minute circumstance!

First stage of the 3d period

In the first stage of the third period the radicle, grown too large for its prison, bursts the seed, and comes forth at the hilum or opening: which, defended by the double cuticle, will only admit of a certain aperture in some seeds, while in others it divides the lobes.

I shall not enter into a thorough detail of the manner, in which the embryo rises and turns to leave the seed, as I have given an exact description of it in my former letter; entering very minutely into every particular of that phenomenon; but proceed to the last stage, which concludes the whole history of the embryo, fixing it in the earth, and raising its head on high. The seed lobes however continue fastened to the fresh plant, lest any accident should happen to the root: for should this be the case, the nourishing vessels, still remaining on the holders, would reassume their office, regain their former fulness, and with the help of the albumen (of which the seeds still retain a certain quantity) nourish the young plant, till the radicle had recovered strength sufficient again to supply its place. I have so often proved this fact by severing the new root to try its effect on the embryo, that I am well assured of its reality: as it never failed to produce these consequences.

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I could have diversified this account, and perhaps made it more amusing, instead of a dry detail of facts: but I write merely to show the *truth*, and I wished particularly to confine this account to what happens to seeds *in general*, rather than to the seeds of any particular plant; that it might in some measure clear up the errours I so much lament: there is nothing more therefore to show, than that, as the radicle (which is always the *tap root*) touches the earth, the nourishing vessels decay; and the primordial leaves raise themselves with the stalk in a perpendicular posture. Seeds differ at

The radicle always the tap root.

Difference in seeds at this time.

this time in one respect. In seed leaves they raise themselves, and continue to grow with the stalk; though they show by their outward form, that they have differed in their first manner of growing; while other seeds, having the silvery and delicate cotyledons, leave them in the earth, where they decay with the lobes. I cannot agree with Mirbel, that they afterward nourish the plant; in a very short time they (as well as the remnant of the seed) break off, and are lost in the earth; or serve as a nest for some of the numerous insects, that equally receive their nourishment from the bounty of the Almighty.

Seed of the chestnut.

Without cotyledons.

Reason of this.

Use of the cotyledons.

I shall now show the conclusion of the chestnut and grass, the former of which differs in a curious manner. It possesses, like the peach and every other seed, the parts already mentioned, except the cotyledons, which it is wholly without. It is impossible to have watched it more narrowly, and to have magnified it to a greater degree; but it has certainly no cotyledons; and the reason why it has none is very plain, and shows distinctly the use of the cotyledons. It has such a length of stalk to the leaf, that the seminal leaves (without greatly exceeding their usual size) could not cover it. The primordial leaves therefore, with their stalk, shoot from the place the cotyledons usually show themselves at; and the stalk of course comes from the same. See the heart of the horse chestnut, fig. 15. This very plainly shows, that the cotyledons are of no other use to the embryo, than screening the primordial leaves from the light and air at their first formation. The esculus differs in no other manner from the peach in its seed; the holders are longer, but they in reality are no more connected with the interior, than the fruit; which I have purposely avoided mentioning, not to confound it with what nature esteems of so much more consequence.

Grass.

As to the grass, it will at first sight appear to possess a part different from other seeds. After strict *examination however*, this is found not to belong to the seed, but to the valves of the grass, and to be the excrescence on which the stamens grow: and as to the small head on which the cotyledons rest, it is certainly a part of the heart, since all the vessels pass through it; and literally is that part, which is first *formed, in the grass ſeeds.* I shall say something more of this order of plants, when I come to explain the formation of the palms.

Many mistakes in this part of botany

The holders of the seed taken for the commencement of the radicle.

Formation and growth of the seed.

Grasses.

Why I have troubled the reader with this long and I fear tedious account has been mentioned: without a minute detail it is impossible, to clear up the innumerable mistakes, that have involved this part of botany in one cloud of errour; nor can they be too soon rectified. The first I shall notice is the supposing the *holders of the seed* (or those parts which retain the lobes, and fasten them to the embryo plant) to be the commencement of the radicle; an explanation universal among phytologists. But this mistake could never have been made, if the seed had been dissected *progressively.* The corculum, which is the first part formed of the embryo, as I have already shown, is the centre of the vessels; the stem and cotyledons shoot from the lower part in the pocket; the radicle from the other end; while the line of life, or impregnating duct, runs through it, in one undeviating thread. But instead of this simple progress it is said, that the radicle, avoiding this direct line, shoots from two different spots in the seed. How is the vessel, that must accompany it, to get there? As well might the tail and hind legs of a chicken be supposed to proceed from the string that fastens it to the egg; nor are the holders of half the consequence the string is to the bird, for that is the vehicle of nourishment to it, but the holders are merely an elongation of the seed vessel for the purposes before mentioned. The seed is merely a box where life is sheltered, but which is only kept from decay by the living embryo it contains; nor is it linked to its cradle till the last epoch; when the holders lengthen, and fasten themselves to the embryo plant, the cuticle enclosing it about an inch down the root, in order more securely to retain it. But as there are few seeds that are thus formed, and the lobes themselves with their outward cuticle *embrace the embryo* in almost all plants, it may well be believed the *part* can be but of *little conſequence.* I know but one order, that gives the least sanction to the idea, and that is the grasses; and the cause of this appearance arises from the radicle occupying not above half the width of the corculum on which it is set; while the nourishing vessels, taking advantage of this, swell out at the other side, giving the whole so crooked an appearance in many species as to be favourable to the supposition. But an unanswerable argument against it is, that not one of the vessels, the radicle must contain, in order to perform its various offices of secretion, impulsion, &c., is to be found in the holders, but all in the heart. This is truly the seat of life, or vital part of the embryo, for the time of its infancy: whereas the holders are a mere elongation of the seed lobes (which is easily proved by dissection) and a thick, strong, dry skin, till the nourishing vessels run on them; when they appear more moist, and increase in length very greatly about the time the embryo leaves the seed. But the view of the drawings will prove the mistake sooner than all my arguments perhaps; and that I may not be accused of favouring my subject by my sketches, I shall borrow one from that excellent work of Dr. Smith's, it is an exact figure of the bean, and plainly shows the holders in their proper light. Dr. Smith not being his own dissector will account for his being also implicated in the mistake, for he marks the holders as the *beginning* of the *radicle*. Mirbel and Wildenouw were also of this opinion. Duhamel, after calling them by a name synonimous to holders, seems to forget it, and finishes by marking them as the commencement of the root. Discouraged at finding so many great men against me, I had scarcely the resolution to seek in Grew for his opinion; and was really *delighted* to find, that he thought the holders of so little consequence, as but just to mention their retaining the lobes; without giving any reason for it. But no person can be deceived who will take a peach, cherry, bean, grass, or any kind of seed, and draw off the lobes; for they will find no resistance to the separation; and a piece of thin skin will be seen to have covered the radicle a little way down, and be very easily divided from it, breaking no vessels whatever.

The holders.

Mistaken for the beginning of the radicle by most botanists, except Grew.

The next mistake I shall endeavour to rectify is that, which supposes the cotyledons to nourish the young plant; without recollecting, that they are a part of the embryo, and cannot therefore nourish themselves: a system absolutely contrary to the laws of nature. What, on this supposition, would

Cotyledons erroneously supposed to nourish the young plant.

would be the use of the nourishing vessels, which occupy towards the last epoch of the seed so large a portion of it, as plainly to evince their consequence? And what should cause these vessels to remain attached to the embryo, but the accidents to which the root is liable? We know indeed, that there is a spot in the seed, which adds to the juices of the nourishing vessels that *sweet fluid* requisite to the support of the young plant. In these, when mixed together, it reposes, as in a bath, sucking them in at every pore: but this sweet fluid alone would not suffice; a less cloying liquid is wanting, and *this* these *important nourishing vessels* produce. Where then is the use of farther support? The cotyledons are often extremely full, juicy, and thick; and (especially in seed leaves) grow extremely fast: consequently, instead of nourishing other parts, they require for their own growth much support. In firs, where there are so many cotyledons (as almost all the pines have 8; indeed I know but two that have 4 only; though they do not all come to perfection), it must require a quantity of juices to form hem, instead of assisting to form the embryo. Nor could ver perceive any diminution in the cotyledons, though I e watched them with the greatest care. When they the seed, they are just as thick as ever, and altered heir green colour; whereas the spot in the seed, roduces the sweet fluid just mentioned, shows, by he embryo leaves the seed, so large a vacancy, as ndicate, that, if the embryo did receive nourishhe cotyledons, these delicate leaves would pro- charity in a very conspicuous manner, having little their *proper form* after such a reduction. Besides, in hose seeds, where it was possible to do it without destroying the primordial leaves, which always greatly hurts the embryo, such as the orange, lemon, &c., I have repeatedly severed the cotyledons the moment they were formed, and it produced no visible effect; though, if it had in the least lessened the food of the embryo, so little can it bear such a privation, it would have died directly.

Nourishing vessels, reproduced as often as cut off.

There are innumerable convincing proofs of the power of the nourishing vessels, and one of the strongest is, that you cannot deprive the seed of them, for they increase as fast as you cut them off. The quantity of hairs that will replace a dilapidated piece in one night is really wonderful. Here therefore Nature plainly speaks her purpose: nor does she less pointedly make it known, when the nourishing vessels decay as soon as the radicle enters the earth. It is such indications as these of the laws and customs of nature, that should be collected by Botanists with care, after being thoroughly verified, and form their axioms, for they cannot mislead. But those which place nature in an unnatural situation, in order to ascertain her rules, I would ever reject; or keep them for trials only, and not build systems on them: for, if the foundation is not secure, how can you trust to the building? I shall say no more on this subject; as a little consideration must I think show, that the idea of the cotyledons nourishing the embryo is a mistake, which will I trust be rectified. I shall proceed therefore to my last subject, grieved that my letter has unavoidably spread to such a length.

but only when the seed takes root.

To the division most physiologists make of monocotyledonous and dicotyledonous plants I should have no manner of objection, were it not founded on the mistaken supposition of there being plants, that have only one cotyledon, which is undoubtedly false. The palms, at least all I have been able to procure for dissection, have 2, and the grasses either 2 or 4. The orchises are so very diminutive in their seeds, that it is not easy for any one to dissect them; but I have been fortunate enough to detect one in a state, that showed its cotyledons in the solar microscope. It is a seed leaf, and has two. The only mosses I have been able to dissect on the same account have visibly two little round leaves from the interior of the pocket, being the usual place; whereas the grasses and palms have their cotyledons rising from the side of the heart, instead of the middle; and what has been taken by botanists for the cotyledons is the primordial leaf, which must of course in both those plants be single, since they grow only leaf by leaf. In the palm, (see Pl. II, figs. 27, 28, 30,) the cotyledons surround the heart, and are indeed very difficult to be separated from it. Nothing but boiling will do, and then it must be the corculum alone that is boiled. The leaves then peel off, and show their number.

The division of plants into monocotyledonous and dicotyledonous a mistake, as there are none with a single cotyledon.

Palms

Orchises.

Date palm.

In the part from which the pocket stretches the primordial leaf shoots, at least in the phoenix dactylifera. There are some very extraordinary things belonging to this seed: it has the appearance of being formed of a collection of extremely diminutive palm-leaves, coagulated and pressed together into so hard a substance, that, when cut into very thin slices with the *wood-cutter*, it presents a picture of pieces of palm leaves with all their veins and vessels. It is a long seed, and rolled, with a deep incision down the middle; and having the appearance of a very diminutive leaf, but very broad, opposite to the corculum, which lies also in the middle of the seed, a very unusual circumstance. See Figs. 26, 27. The corculum is uncommonly large and white. The little wild palm has a still larger heart; the seed is round; it has two cotyledons spreading round it in the same manner as the other palm; and the wax palm has also two cotyledons, but thinner, whiter, and more delicate. I have planted them, and hope in my next to show their appearance when leaving their seed, which, as they require time, I have not yet been able to do.

Little wild palm.

Grasses.

As to the grasses, the reason that all physiologists have joined in refusing the name of cotyledons to those diminutive leaves, which have all the appearance of seminal leaves, and *certainly* perform *all the functions of them*, such as screening the primordial leaves, &c., has been, I suppose, because, instead of appearing at the middle of the bottom part of the cotyledons, they appear at the two sides; but they are undoubtedly the cotyledons, and the leaf, which has falsely been called so, is the primordial leaf, and proves itself to be *this*, by showing a complete grass leaf, exactly the same as those which succeed it. The cotyledons are diminutive, thin, silvery leaves, that screen the primordial ones; and should, I think, be restored to their original denomination.

Division of plants.

There can be no doubt, that the division of plants is an excellent one; but it would be quite as good, when founded on the primordial leaves being single, as the cotyledons being so. I have had some thoughts of arranging the seeds in such a manner, that a word or two added to the present general description should indicate what sort of seed the plant had; whether a *leaf seed*, a *rolling seed*, or a *common seed*; making these the order, with genera and species, exactly to discriminate the sorts: for is it proper, that the exterior of seeds should be described in so elaborate a manner, and that the *interior*, by far of the most consequence, that part which Nature has distinguished with every attention and every care possible, should be wholly neglected? Would it not be extremely curious to inquire what effect a plant derives from being a *seed leaf?* For, though a seed leaf begins exactly like a common seed, and has all the eight parts before mentioned, it differs very greatly in one respect; that is, when the pocket is complete, and joins the heart, the cotyledons grow with a quickness impossible to describe, and have also additional means of nourishment for this purpose, and for the growth of those vessels, which, like common leaves, are regularly wove, elongating from the back, and brought for this purpose from the exterior. This must cause a great change in the plant, I should conceive. Nor would it be less curious to see the effect of the *rolling seed* on the plants. But I shall leave these subjects to be discussed in my next, should this be received with the same degree of favour I have before been honoured with.

I am, Sir,

Your obedient humble servant,

AGNES IBBETSON.

Division of seeds.

P. S. Most succulent herbaceous plants have leaf seeds; most strong and vigorous trees have *common seeds*; and the rolling seed, which is a different sort of leaf seed, generally indicates a weak small plant, such as climbers, and creeping plants. I again repeat, it makes little difference in the seed, nor is it possible to tell what the seed will be, till nearly the end of the second period; but this difference I shall explain in my next. I have now dissected eight different sorts of mosses, and they have all two seminal leaves, and so have the tremella and the lichen.

Explanation

Explanation of the Plates.

Explanation of the plates.

Plates I and II. Fig. 1. The seed vessel of the horse chestnut, cut open horizontally. *a, a, a, a,* the impregnating vessel, or vessel of life, proceeding from the interior of the seed vessel. *k, k,* the string that stretches to attain the heart, and which, I think, is mistaken for the radicle. *b, b, b, b,* the nourishing vessels (marked by dotted lines only, to distinguish them), which always proceed from the exterior of the seed vessel. *d, d, d, d,* the pocket.

Note. Similar letters of reference denote the same parts in all the figures.

Fig. 2. Half a similar seed vessel cut longitudinally, showing the first appearance of the seed vessel in the bud of a female flower.

Fig. 3. The seed of the horse chestnut, Fig. 4, that of the peach, and Fig. [illegible], that of a gr[illegible], as they first appear in the bud of the flower. *c,* the corculum. *e,* the cuticle, or outer skin of the kernel. The seed of the peach is delineated in the seed vessel: the others are taken out of it.

Figs. 6, 7, 8. The same seeds in their second stage of growth.

Figs. 9, 10, 11. The three seeds in their third stage, when impregnated; the pocket joining the corculum, and the string *k* disappearing.

Figs. 12, 13, 14. The seeds in their fifth stage: the corculum perfected, the seminal leaves almost complete, and the nourishing vessels on both sides of the seeds.

Figs. 15, 16, 17. The three plants, showing only the embryo of the chestnut and peach. This is now as complete as it ever is, till placed in the ground in the seed. It is given thus, to show, that there is no radicle to it; and that the root, which will grow as soon as it is placed in the ground, can proceed only from *x*. This is easily seen, by comparing these with the plants where the root is annexed, which is merely filled out, and grown longer; and where the heart is still to be found, marked by a dotted circle. *g, g,* the cotyledons. *i, i,* the primordial leaves.

Figs. 18, 19. The bean and its embryo from Dr. Smith. *g,* the cotyledons. *h,* the holders; which show how little they

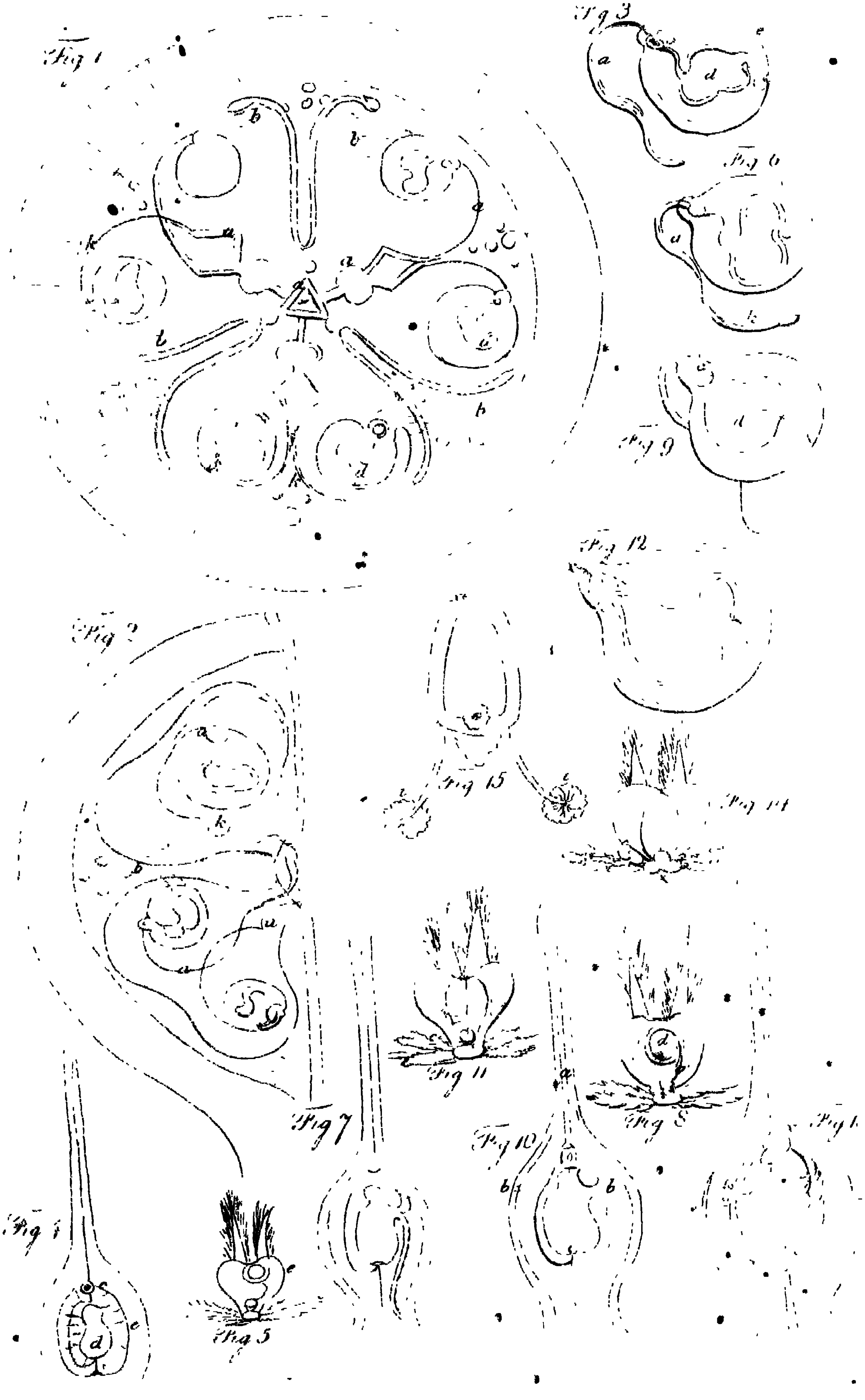
Fig 1
b
b
a
a
k
b
b
d
d
e
Fig 3
a
d
e
Fig 6
a
k
Fig 9
d
Fig 12
Fig 15
Fig 14
Fig 2
a
k
b
a
Fig 11
Fig 8
d
Fig 7
Fig 10
a
b
b
Fig 1
Fig 4
d
e
Fig 5
e

they can pretend to be the origin of the root. This is farther evident from

Figs. 20 and 21: the former showing a French bean, with the part to which the radicle grows starting from it: the latter, the same magnified: *z*, the part on which the cotyledons grow; *y*, that from which the radicle proceeds; *h*, *h*, the holders.

Figs. 22, 23, 24, 25. The chestnut, peach, grass, and bean; showing the completion of the embryo by its growing in the earth. *f*, the radicle. *u*, the seed.

Fig. 26. The seed of the date palm.

Fig. 27. The heart greatly magnified, with the leaves, or cotyledons, wrapped round it.

Fig. 28. The same, with the leaves unfolded, to show that they are two, and that the point leaf is a primordial leaf.

Fig. 29. The seed of the little palm.

Fig. 30. The heart, with its two cotyledons unfolded.

II.

On the Ratio the spontaneous Evaporation of Water bears to Heat: by HONORE FLAUGERGUES[*].

Prize question proposed.

THE celebrated academy at Lyons proposed last year as the subject of a prize, " to determine the relation between the spontaneous evaporation of water and the state of the air, as shown by the thermometer, barometer, and hygrometer." This interesting question I was tempted to investigate; and accordingly I began a series of experiments as early as the month of September, 1806, which I have since continued without interruption. The academy was very indulgent to the paper I had the honour of transmitting it on this subject: but the prize it condescended to bestow on me I consider less as a reward, which I was far from meriting, than as an excitement to multiply and extend my researches. I have therefore continued the inquiry I had begun, so that my work has reached a considerable extent;

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[*] Journal de Physique, vol. LXV, p. 446.

and as it cannot be published entire, I have thought I should gratify the lovers of natural philosophy, if I extracted from it what relates to the relation between evaporation and heat, giving it in as concise a form as possible.

Is evaporation proportional to the surface exposed simply?

Before I entered into the particular investigation of the changes, which the state of the air occasions in evaporation, I thought it would be right to examine the general law, which it follows in all cases, and endeavour to decide the grand question, whether, as most philosophers think, evaporation be proportional to the extent of surface of the water in contact with the air; or whether it depend also on some function of the other dimensions of the body of water exposed to evaporation, as Muschenbroeck* and Côte† assert.

Under similar circumstances it is.

With this view I have made a number of experiments; and, after having varied them in all ways, I have constantly found, that, under similar circumstances, evaporation is precisely proportional to the extent of the surface of the water in contact with the air. I found too, that, when these two gentlemen imagined they had observed the existence of another law, it was because, from the arrangement of the vessels employed in their experiments, the water contained in them was heated and cooled unequally, whence arose accidental variations of the evaporation, concealing the true law; and which would not have taken place, if these vessels had been placed in air of a constantly uniform temperature, or if they had been surrounded with a large body of earth, or some other substance, as Mr. Sedilleau long ago observed‡.

Measure of evaporation.

When we find, that evaporation, under similar circumstances, is proportional to the surfaces, we require nothing more, to express its measure, than the number of lines the surface of the water is lowered by this evaporation in a given time. For this time I have taken four and twenty hours, or one day.

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* Essays on the Experiments of the Academy del Cimento, Tome I of the Academical Collection, part. étr. p. 142.

† Journ. de Physique, vol. XVIII, p. 306.

‡ Anc. Mém de l'Acad. des Sciences, tom. X, p. 33.

Experiments to ascertain it.

The experiments I made to determine the relation of evaporation to heat were very simple. On a table in the middle of a large room, the air of which was perfectly still and heated to a given temperature, and the humidity of which was also ascertained and constantly the same, I placed cylindrical or prismatic vessels of glass and metal, the diameters of which were of no importance; but I made them all above an inch, because evaporation goes on less freely in vessels with small apertures. These vessels I filled with spring water heated precisely to the temperature of the room, and noted the time when the experiment commenced. Keeping the air of the room uniformly at the same temperature, when I thought the quantity of water evaporated might begin to render the air sensibly moister, and thus diminish its solvent power, I measured how much the surface of the water in the vessels was lowered. The time of taking this measure, which was that of the conclusion of the experiment, I noted down; and by the rule of proportion I found what the evaporation would have been, if the experiment had continued twenty-four hours.

Difficulties.

These experiments, though very simple, are attended with some difficulty, if well executed. It is not easy, to keep the air of a large room at the same temperature for any length of time, or to have it always at the same degree of humidity. By care, however, I accomplished both these points for a time sufficient to be perfectly sure of the results of these experiments.

Instruments employed.

To determine the degrees of heat I employed several excellent thermometers, constructed on the principle of Mr. De Luc, and two of which were made by the late Mr. Paul of Geneva. I was not equally happy in my means of ascertaining the degree of humidity; for whatever pains I took, I could not procure one of Mr. Saussure's hair hygrometers. This instrument, considered as the most accurate, or least defective, of the kind, is very difficult to be met with since the death of this celebrated artist, who was perhaps the only person that succeeded in making good ones. But as all that was requisite in the present instance was to find one constant degree of humidity, I endeavoured to supply their want by means of hygrometers made of one piece of gut, but which I constructed with particular attention, as I may perhaps relate at large on some future opportunity. I shall only say, that the uniform degree of moisture I chose for my experiments answered nearly to 50° of de Saussure's hygrometer *.

Measurement. To measure the lowering of the surface of the water in the vessels, I employed a scale of 1000 equal parts, accurately divided by Canivet. Of these parts 190 were precisely equal to a French inch. I took the measure on the side of the glass vessels with a pair of spring compasses, the points of which were extremely fine, and a lens. For those of metal I employed a small commodious instrument. It consists of a capillary tube of glass, firmly fastened at right angles to a wooden ruler perfectly straight; and a scale similar to the preceding traced on a very thin and narrow slip of brass, fixed to the tube. The inside of the tube being wetted by a drop of water introduced into it, the tube is immersed perpendicularly in the water of the vessel, till the edge of the ruler rests on the brim of the vessel. The water ascends in the tube by capillary atraction, and the point of the scale to which it rises at the commencement of the experiment is noted down. The same operation is repeated at the conclusion of the experiment; and the difference between the former point and that to which it now rises measures the lowering of the surface by evaporation.

Law of evaporation. When I had thus obtained five or six well defined evaporations, corresponding to equal intervals expressed in degrees on the scale of the thermometer, or to equal differences of heat, I endeavoured to find the law these evaporations followed. For this purpose I made many fruitless trials, till an idea suggested itself, which from its simplicity ought to have presented itself at first; that of introducing between the two extreme evaporations as many geometrical proportionals as there were evaporations observed between them.

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* In my experiments on the ratio of evaporation to the moisture of the air I employed a method of determining this moisture more certain than the most perfect hygrometers; that of calculating directly the quantity of water in vapour contained in a given bulk of air, by absorbing this water by perfectly dry potash.

them. Having done this, I found to my great satisfaction, that these mean geometrical proportionals evidently represented the intermediate evaporations. All the experiments I afterward made, the results of which are given in the following table, confirm this law.

Explanation of the table.

The first column of this table contains the degrees of Mr. De Luc's thermometer *, at the temperature of which I made my experiments †.

The third column contains the mean results of 291 experiments, which I made to determine the ratio of evaporation to heat, and the degree of spontaneous evaporation of water at every degree of Mr. De Luc's thermometer from 0° to 31°. To give the particular result of every experiment would have been attended with little advantage, and occupied too much valuable room: accordingly I have divided the sum of the evaporations observed in every experiment under the same degree of heat by the number of these experiments, and given only the quotient, or mean result.

The fourth column contains the evaporations calculated according to the rule above mentioned; that is, by inserting 30 mean geometrical proportionals between the numbers expressing the evaporations observed at 0° and 31°.

The fifth column gives the difference between the evaporations thus calculated and the mean of those observed.

In making these experiments I chose times when the barometer was about its mean height, which I determined by 1600 observations at my observatory, each day at noon, to be 27 inches, 9·3 lines [29·56 in.], supposing the quicksilver at the temperature of melting ice.

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* A second column is here added, containing those of Fahrenheit.
C.

† I made some other experiments indeed, and as far as 40° of the thermometer [122° F.]; but as I have not yet been able to repeat them as often as I wished, I have not reported their results.

Thermometer.

Table of the spontaneous evaporation of water at different temperatures.

Thermometer.	Fahrenh.	Daily evaporation observed.	Daily evaporation by the rule.	Difference.	Thermometer.	Fahrenh.	Daily evaporation observed.	Daily evaporation by the rule.	Difference.
0°	32°	4·4	4·4	0	16°	68°	17·8	18·7	+ 0·9
1	34·25	4·5	4·8	+ 0·3	17	70·25	19·4	20·5	+ 1·1
2	36·5	4·4	5·3	+ 0·9	18	72·5	22	22·4	+ 0·4
3	38·75	5·9	5·8	— 0·1	19	74·75	24·8	24·6	— 0·2
4	41	6·8	6·3	— 0·5	20	77	27·3	26·9	— 0·4
5	43·25	7·3	6·9	— 0·4	21	79·25	30·2	29·4	— 0·8
6	45·5	7·4	7·6	+ 0·2	22	81·5	32	32·2	+ 0·2
7	47·75	8	8·3	+ 0·3	23	83·75	35·6	35·2	— 0·4
8	50	9·6	9·1	— 0·5	24	86	38·9	38·6	— 0·3
9	52·25	10·3	9·9	— 0·4	25	88·25	42	42·2	+ 0·2
10	54·5	10	10·9	+ 0·9	26	90·5	46·8	46·2	— 0·6
11	56·75	10·9	11·9	+ 1	27	92·75	51	50·6	— 0·4
12	59	13·2	13	— 0·2	28	95	55·7	55·4	— 0·3
13	61·25	14	14·3	+ 0·3	29	97·25	61	60·7	— 0·3
14	63·5	15·9	15·6	— 0·3	30	99·5	66·9	66·4	— 0·5
15	65·75	16·4	17·1	+ 0·7	31	101·75	72·7	72·7	0

The differences owing to inaccuracies in the experiments.

If we examine the differences in the fifth column, we shall readily perceive, 1, that these differences are very small; 2, that they are indifferently positive or negative; and, 3, that their sum is next to nothing: whence it follows, that these slight differences may reasonably be ascribed to the errours unavoidable in experiments of this kind; and that, without these errours, the evaporations observed would have coincided with those calculated according to our hypothesis, which may consequently be considered as perfectly conformable to nature.

Evaporation increases in a geometrical progression

From the preceding experiments therefore we may infer this remarkable law, that, while the degrees of heat increase or diminish in arithmetical progression, the corresponding evaporations increase or diminish in geometrical progression. Thus, the heat in our experiments increasing uniformly and successively one degree [2·25° F.], the corresponding evaporations form a geometrical progression, each term of which is to the preceding in the ratio of 1·0947 to 1. So these evaporations form a geometrical progression nearly in a duplicate ratio, if we take intervals of 7·6° [17·1° F.]; and nearly in a triplicate ratio, if the intervals be of 12° [27° F.].

Formula.

Hence it follows, if we suppose, that the degrees of the thermometer are represented by equal parts of a right line, and that on each of the points corresponding to a degree we erect a perpendicular equal to the evaporation that answers to that degree of heat, the degrees of the thermometer will be the abscisses, and the corresponding evaporations the ordinates of a logarithmic curve, the subtangent of which may be found by the following ratio.

As 2·8047369 (the difference of the Naperian logarithms 1·4816045 and 4·2863414, answering to the numbers 4·4, and 72·7) is to 31 (the difference of the correspondent abscisses 0 and 31), so is 1 (the subtangent of the logarithmic of the Naperian system) to 11·0527301 (the subtangent of the logarithmic of the evaporations).

The equation of the logarithmic, putting x for the abscise, y for the ordinate, and S for the subtangent, is $S\,dy = y\,dx$. If we sum up this equation; complete the integral, remembering, that $x = o$ gives $y = \log.\ (4{\cdot}4)$; and reduce it to numbers, putting for S the value found above; we shall ultimately have the equation

$$y = (4{\cdot}4)\,.\,(2{\cdot}7182818)^{11{\cdot}0527301}$$

In which equation x represents the degree of Mr. De Luc's thermometer given, and y the corresponding evaporation expressed in parts of my scale of 1000 equal parts. If we would have the evaporation in millimetres, this value may be multiplied by $\frac{27{\cdot}07}{190}$, or the number 0·6268843, may be substituted in the equation instead of the coefficient 4·4 *.

Property leading to a knowledge of the nature of evaporation.

From the nature of the logarithmic, if we suppose dx constant, we shall have dy proportional to y: whence we may infer, that, the increments of heat taking place by infinitely

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* To have the evaporation in English inches, this value should be divided by 178·273, the number of parts in the scale of Mr. Flaugergues equivalent to an English inch; or 0·0246812472 substituted instead of the coefficient 4·4. C.

finitely small and equal degrees, the corresponding increments of evaporation are proportional to the evaporation itself, a singular property, and which, it seems to me, may lead to a more accurate knowledge of the nature of evaporation, and decide between the two celebrated systems of Leroy and Dalton, which at present divide the suffrages of natural philosophers.

III.

Method of ascertaining the Value of Growing Timber Trees at different and distant Periods of Time: By Mr. CHARLES WAISTELL, *of High Holborn**.

SIR,

Method of ascertaining the value of growing timber.

CONCEIVING, that the Tables contained in the annexed papers will afford useful information to growers of timber, and tend to encourage the growth of it in these kingdoms, and hereby promote the views of the Society of Arts &c., I trust you will have the goodness to lay them before the Society, as I have formed them with great attention.

Having last autumn viewed some plantations made under my direction about thirty years ago, I found the value of one of them much to exceed my expectation. I became therefore desirous to devise some means of estimating what its value might probably be at different future periods. I was thus led to construct the first of these tables, and on the completion of this, other tables seemed necessary, and I was thus progressively led on to the construction of the whole. For this purpose I searched in various authors for the measure of trees, in girth and height, at different ages, and obtained similar information among my acquaintance. Hence I collected, that the increase in the circumference of trees is generally from about one to two inches annually, and

General increase of trees in height and girth from twelve to eighteen inches the annual increase in height. Some fall a little short, and some exceed these measures.

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* Trans. of the Soc. of Arts, vol. XXVI, p. [illegible] The gold medal of the Society was voted to Mr. Waistell for this communication.

Use of the first table,

I shall now briefly notice a few of the advantages to be derived from the first Table.

trees should not be felled too soon,

1st. The first table shows every fourth year, from twelve years old to a hundred, the rates per cent per annum at which all trees increase, whether they grow fast or slow, provided their rate of growth does not vary. This table may be the means of saving young thriving woods from being cut down, by showing how great a loss is sustained by felling timber prematurely[*].

or stand too long.

2d. And it may be the means of bringing old trees to market, by showing the smallness of the interest they pay for the money they are worth, after they are 80 or 100 years old.

Trees decrease before it is apparent.

But this table shows the interest which they pay, only, while the trees continue growing at their usual rate. In case they fall short only a little of their usual increase in girth, this considerably diminishes the rate per cent per annum of their increase. And trees do decrease in their rate of growth, before they appear to do so[†]. A pale and mossy bark are certain indications of it.

Signs of this.

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[*] "A wood, near West Ward, in Cumberland, of more than 200 acres, was felling in 1794, it was little more than 30 years old. The whole was cut away without leaving any to stand." *See Miller's Gardener's Dictionary, last edition, under the Head of Woods.*

At 30 years old timber pays 10 per cent for standing, and probably this wood might have paid 7 per cent per annum on an averge for the next 30 years.

[†] In Mr. Pringle's Agricultural Report for Westmoreland is a paper of the Bishop of Llandaff's, stating, "That a very fine oak, of 82 years growth, measured in circumference at 6 feet from the ground, on the 27th of October 1792, 107 inches, and on the same day of the same month in 1793 it measured 108 inches." He then states the interest it paid to be only about 2 per cent, and says this tree was a singularly thriving one. It is evident, that, with all this appearance of thriving, it was on the decline. For if we divide 108, its inches in circumference, by 82, its age, we find its average annual increase had been 1 inch and a third. Its falling off to 1 inch reduced the rate per cent of increase one fourth.

3d. The first Table may also assist the valuer of such timber as is not to be cut down, but to continue growing, by enabling him to estimate its present value more accurately than is usually done, especially when it is increasing after a high rate per cent per annum*.

2d table.

The 2d Table shows the rate per cent to be the same as in the first Table, though the annual increase is more both in height and circumference.

3d table. Distance of trees.

The 3d Table is calculated to show the number of trees that will stand on an acre of ground, at the distance of one fifth of their height, (which distance is recommended by Mr. Salmon, in a paper in the Society's 24th volume,) and the number of feet the tree will contain, both those to be cut out, and those to be left standing, at the end of every four years, from 16 to 64 years old, supposing they increase 12 inches in height and 1 in circumference annually. This distance may suit fir trees, but will be too near for oaks.

4th and 5th tables.

The 4th and 5th Tables show the same particulars when the trees grow at greater rates

6th table.

The 6th Table is calculated to show the same particulars when the trees are constantly thinned out every four years, so as to leave them at the distance of one fourth of their height. According to this table there will be 48 trees left on an acre when they are 120 years old; and it seems generally agreed, that from 40 to 50 full grown oak trees are as many as have sufficient room to stand on an acre.

7th table

The 7th Table shows the same particulars respecting trees which increase 15 inches in height and 1½ inch in circumference annually.

8th table.

The 8th Table shows the same particulars respecting trees which increase 18 inches in height, and 2 inches in circumference annually.

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* A fir wood of more than 30 acres, and about 20 years old, was lately valued to be sold with an estate, by several eminent wood valuers, without taking into consideration its rate of increase. It was then increasing after the rate of 10 per cent per annum, and probably would increase after the rate of 8 per cent on an average for the next twenty years.

The 9th Table shows the same particulars as Table 6, till the trees are 28 feet high, after which the distance is increased from one fourth to one third of their height.

9th table.

The 10th, 11th, and 12th Tables show the annual increase in boles of 24, 32, and 40 feet long, and the difference of their increase at the same ages.

10th, 11th, & 12th tables.

To these tables succeed comparative statements, showing the number of feet contained in boles of different lengths, when the trees are 60 years old, by which it appears, that, if cut down at that age, the longest boles are not the most profitable to the growers of timber.

Comparison of boles of different lengths.

And I have added the valuation of the plantations before alluded to, with remarks on them.

Having finished my introductory remarks, I conclude, and am, Sir,

Your very humble servant,

CHARLES WAISTELL.

Tables respecting the Growth of Timber.

Calculations, showing every fourth year, from 12 to 100, the progressive annual increase in the growth of trees, and gradual decrease in the rate per cent per annum, that the annual increase bears to the whole tree.

Tables respecting the growth of timber.

The whole height of the trees is taken to the top of the leading shoot, and the girt in the middle; but no account is taken of the lateral branches.

If trees increase 12 inches in height and 1 in circumference annually, their increase will be as follows, viz.

TABLE.

TABLE I.

Years old & ft. high.	Girt.	Contents.			Years old & ft. high.	Girt.	Contents.				One year's increase.				Rate per cent of increase.
	inch	ft.	in.	pts.		inch.	ft.	in.	pt.	sds.	ft.	in.	pt.	sds.	
12	1½	0	2	3	13	1⅝	0	2	10	3	0	0	7	3	26·8
16	2	0	5	4	17	2⅛	0	6	4	9	0	1	0	9	19·9
20	2½	0	10	5	21	2⅝	1	0	0	8	0	1	7	8	15·7
24	3	1	6	0	25	3⅛	1	8	4	1	0	2	4	1	13·
28	3½	2	4	7	29	3⅝	2	7	9	1	0	3	2	0	11·
32	4	3	6	8	33	4⅛	3	10	9	6	0	4	1	6	9·67
36	4½	5	0	9	37	4⅝	5	5	11	5	0	5	2	5	8·5
40	5	6	11	4	41	5⅛	7	5	8	10	0	6	4	10	7·6
44	5½	9	2	11	45	5⅝	9	10	7	9	0	7	8	9	6·96
48	6	12	0	0	49	6⅛	12	9	2	3	0	9	2	3	6·38
52	6½	15	3	0	53	6⅝	16	1	10	2	0	10	10	2	5·9
56	7	19	0	8	57	7⅛	20	1	1	7	1	0	5	7	5·4
60	7½	23	5	2	61	7⅝	24	7	6	6	1	2	4	6	5·1
64	8	28	5	4	65	8⅛	29	9	7	0	1	4	3	0	4·76
68	8½	34	1	4	69	8⅝	35	7	8	11	1	6	4	11	4·49
72	9	40	6	0	73	9⅛	42	2	6	4	1	8	6	4	4·2
76	9½	47	7	6	77	9⅝	49	6	5	2	1	10	11	2	3·98
80	10	55	6	8	81	10⅛	57	7	11	9	2	1	3	9	3·79
84	10½	64	3	8	85	10⅝	66	7	7	8	2	3	11	8	3·6
88	11	73	10	4	89	11⅛	76	5	11	1	2	7	7	1	3·5
92	11½	84	5	9	93	11⅝	87	3	4	0	2	9	7	0	3·3
96	12	96	0	0	97	12⅛	99	0	4	6	3	0	4	6	3·15
100	12½	108	6	0	101	12⅝	111	9	6	8	3	3	6	8	3·

Remarks.

In Table X of the increase of a bole of 24 feet in height, of a tree growing at the abovementioned rate, it will be observed, that the contents at 24 years of age are the same, and at 64 years nearly the same as in the above Table, but the contents of the bole at all the intermediate periods exceed the above. And a 40 feet bole exceeds the above contents from 44 years to 100, as may be seen in Table 12. For these reasons chiefly I did not think it necessary to take into consideration the decrease in height that takes place in trees at different ages, according to the kind of tree and quality of the soil.

The increase per cent per annum is the same as the above in all trees at the same age, whether they have grown faster or slower, provided their increase in height and thickness annually has not varied on an average. The progress of trees is sometimes greatly retarded by insects destroying their leaves, by unfavourable seasons, and by their roots penetrating into noxious strata. But these accidents cannot enter into calculations.

Calculations, showing every fourth year from 12 to 64, the progressive annual increase in the growth of trees, and the gradual decrease in the rate per cent per annum that the annual increase bears to the whole tree. Table 2.

The whole height of the trees is taken to the top of the leading shoot, and the girt in the middle; but no account is taken of the lateral branches.

If trees increase eighteen inches in height, and two inches in circumference, annually, their increase will be as undermentioned, viz.

TABLE II.

Age of Trees.	Height.	Girt.	Contents.			Age of Trees.	Height.	Girt.	Contents.				One year's increase.				Rate per cent of increase.
	ft.	In	ft.	in.	pt.		feet	inch.	ft.	in.	pt.	sd	ft.	in.	pt.	sd	
12	18	3	1	1	6	13	19½	3¼	1	5	1	0	0	3	7	0	26·5
16	24	4	2	8	0	17	25½	4¼	3	2	4	0	0	6	4	0	19·8
20	30	5	5	2	6	21	31½	5¼	6	0	3	6	0	9	9	6	15·6
24	36	6	9	0	0	25	37½	6¼	10	2	0	6	1	2	0	6	13·
28	42	7	14	3	6	29	43½	7¼	15	10	6	0	1	7	0	0	11·
32	48	8	21	4	0	33	49½	8¼	23	4	8	0	2	0	8	0	9·6
36	54	9	30	4	6	37	55½	9¼	32	11	7	6	2	7	1	0	8·5
40	60	10	41	8	0	41	61½	10¼	44	10	3	6	3	2	3	6	7·6
44	66	11	55	5	6	45	67½	11¼	59	3	10	0	3	10	4	0	6·9
48	72	12	72	0	0	49	73½	12¼	76	7	1	0	4	7	1	0	6·1
52	78	13	91	6	6	53	79½	13¼	96	10	11	6	5	4,	5	6	5·8
56	84	14	114	4	0	57	85½	14¼	120	6	8	6	6	2	8	6	5·4
60	90	15	140	7	6	61	91½	15¼	147	9	2	0	7	1	8	0	5·
64	96	16	170	8	0	65	97½	16¼	178	9	4	0	8	1	4	0	4·7

Explanation of the Construction of Table I and II.

Construction of tables 1 and 2 explained.

To render the preceding tables easy to be understood by persons not accustomed to calculations, I will state the process of the operations in the first line of Table II.

The height of the tree at 12 years of age is supposed to be 18 feet to the top of its leading shoot, and 24 inches in circumference at the ground, consequently, at half the height, the circumference is 12 inches. One fourth of this, being 3 inches, is called the girth. The girth being squared, and multiplied into the height, gives one foot one inch and six parts for its contents. At 13 years old the tree will be 19½ feet high, 26 inches in circumference at the ground, and 13 inches at half the height, one fourth of 13 gives 3¼ inches for the girth. This squared and multiplied into the height, give one foot five inches and one part for the contents. Deduct from this the contents of the tree at 12 years of age, and there remain three inches and seven parts, which is the increase in the 13th year. Then reduce the contents of the tree when 12 years old, and the increase in the 13th year, each into parts, dividing the former by the latter, and the quotient will be 3·76; by this number divide 100, and the quotient is 26·5, which is the rate per cent of increase made in the thirteenth year. Consequently whatever the tree might be worth when 12 years old, it will, at the end of the 13th year, be improved in value after the rate of 26*l.* 10*s.* per cent; or in other words, that will be the interest it will have paid that year for the money the tree was worth the preceding year.

At every succeeding period, both in this Table and Table I, the like process is gone through.

Observations on Tables I and II.

General observations on the preceding tables.

The preceding tables furnish us with the following useful information, viz.

1st. That all regular growing trees, measured as above, as often as their age is increased one fourth, contain very nearly double their quantity of timber.

2d. That when a tree has doubled its age, its contents will be eight-fold.

3d. That when a tree has doubled its age, the annual growth will be increased four-fold.

4th. Consequently, that when a tree has doubled its age, the proportion that its annual increase bears to the contents of the whole tree is then diminished one half.

This last observation explains how it comes to pass, that a tree, when its age is doubled, the rate per cent per annum that its increase then bears to the contents of the whole tree, is diminished one half.

It may not be unuseful to observe, that the rate per cent of increase in the last columns, is the same as the rate per cent that the increase of the tree that year will pay for the money it was worth the preceding year.

In the two preceding tables, we find that the rate of increase per cent per annum is the same in both at the same ages, although the quantity of timber in the second table is six times as much as in the first table in trees of all ages; therefore, when the age of a tree is known, the rate per cent per annum [illegible]s increase is known on inspecting these tables, whether the tree has grown fast or slow; provided the growth of the tree has been regular, and that it has continued its usual growth.

And having the age, girth, and height of any tree given, we can readily calculate what quantity of timber it will contain at any future period, while it continues its usual rate of growth.

(To be continued.)

IV.

Observations on Potash and Soda prepared with Alcohol: by Mr. d'ARCET. *Read to the Institute the* 11*th of January*, 1808.*

WHEN chemistry, employing new methods of analysis, is enriching itself with important facts; when England announces the decomposition of potash and soda, and the chemists of France are busied in confirming this grand discovery; I conceive it incumbent on me to communicate the results of various experiments, which may probably throw some light on the path newly opened.

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* Annales de Chimie, vol. LXVIII, p. 175.

I present

Importance of facts relative to the fixed alkalis.

I present only facts that still require verification; but they appear to me of the greater importance, as they relate to those alkalis, the decomposition of which has been announced; and are naturally applicable to the analysis of saline substances, an important branch of science, since almost all analytical processes ultimately produce them, and then conclusions are formed from the knowledge we have of the proportions of their elements.

Method of finding the quantity of alkali in the impure sorts of the shops.

Seeking some months ago an easy and speedy method of ascertaining the quantity of pure or carbonated alkali contained in the different sorts of potash and soda found in the shops, I compared the various processes that have been made public, and soon perceived the advantage of those, in which acids are employed to determine the quantity of alkali, and this quantity is found from the weight of acid required to neutralize the mixture.

Sulphuric acid preferred.

The author's method of proceeding.

Various considerations, which it is unnecessary to mention, led me to prefer the sulphuric acid, as proposed by Mr. Descroizilles. I carefully examined his method, and, fully satisfied of its goodness, made the following experiments. I must observe, they were all made with at least 20 gr. [309 grs]; but most of them with 100 gr. [1544 grs]; and that each result is the mean of four experiments, which frequently differed only in the second decimal figure. I began by thoroughly purifying a few kilogrammes of subcarbonate of soda. After having separated by successive crystallizations the small quantity of muriate and sulphate of soda it contained, I reduced the crystals to coarse powder, and left them exposed to a temperature of 12° or 14° [54° or 57° F.], till they were thoroughly dry. I then took some sulphuric acid, carefully distilled, and very pure, the specific gravity of which was to that of water as 1·844 to 1: I reduced its specific gravity to 1·066 by diluting it with 9 parts of distilled water: and this acid, thus diluted, I employed in the course of my experiments. I need not say, that, on dividing its weight by ten, the corresponding quantity of concentrated acid is found, which, expressed in numbers, would represent the strength of the alkali employed to saturate it.

Analysis of

These preliminaries being settled, the analysis of the subcarbonate of soda.

carbonate, which I had prepared, was conducted with all possible care, varied in several ways, and uniformly announced its composition to be

Water	63·61
Carbonic acid	16·04
Soda	20·35
	100·

Considering myself certain of the accuracy of these results, and having paid so much the more attention to the experiments that furnished them, because they were to serve as a standard to the rest, I thought I might take this analysis as a settled point, and then proceeded to the following experiments.

Quantity of sulphuric acid required to saturate it.

Taking the usual precautions, I first neutralized 100 gr. [1544 grs] of the subcarbonate of soda abovementioned. Repeating this experiment several times, the mean term of the results was 347, expressing the quantity of diluted acid required for the saturation, and representing 34·7 grammes of concentrated acid.

Thus I found myself authorized to conclude, that the employment of 34·7 grammes of concentrated sulphuric acid, similar to that of which I have given the specific gravity above, would always represent, at the same temperature, in a solution of soda brought by this acid to the neutral state, 100 grammes of subcarbonate of soda similar to that I had analysed; or, which is the same thing, 36·39 of dry subcarbonate, or 20·35 of pure soda.

Soda purified with alcohol saturated with the same acid.

I then repeated the same experiments, substituting, instead of the subcarbonate of soda, caustic soda prepared with alcohol, hitherto considered as pure soda, and the real standard of this alkali: but I was surprised at the results I obtained; and the conclusions I was compelled to draw appeared to me so contrary to the received opinions, that I omitted nothing to remove every sort of doubt. Accordingly I made a number of experiments, and obtained the following results.

First, I examined four different specimens of soda prepared with alcohol, and simply fused in a silver capsule:

none of these specimens were perfectly pure; all of them indicating slight traces of muriatic acid, and a greater or less proportion of carbonic acid, easily detected by barytic salts, barytes water, lime water, &c., but too little for a solution not very strong to effervesce on the addition of the acid. On neutralizing 20 gr. [309 grs] of each of these specimens, I found, that 100 parts of

Proportions required.

N° 1 had absorbed 110·2 of concentrated sulphuric acid.
2 . . . . 116·75
3 . . . . 111·5
4 . . . . 112·2

This would indicate, taking the mean of the results, that 100 parts of caustic soda required only 112·662 of concentrated sulphuric acid for their neutralization.

The experiments repeated.

Apprehending, that the specimens of soda employed, notwithstanding they had been in fusion, might contain more or less water, I repeated these experiments on similar portions of soda, which had been fused separately in a silver crucible, and kept in this state at a red heat for twenty minutes; but the proportions obtained differed so little from those above given, that it is unnecessary to insert them.

Soda thus prepared therefore not real alkali.

On comparing these results with those before obtained, we must conclude, that, if 20·35 of pure soda, in the subcarbonate analysed, required 34·7 of concentrated acid to saturate them, 100 would take 170·515: but we have just seen, that 112·662 of this acid were sufficient to neutralize 100 of the caustic soda prepared with alcohol; whence it follows, that this soda was not pure, which is probable; or that the analysis of the subcarbonate was erroneous, a supposition that I conceive inadmissible, from the various trials made.

Experiments repeated with soda

Notwithstanding I was satisfied, that the four specimens of soda prepared with alcohol contained too small a portion of any known foreign matter, to occasion so great a difference between the results obtained, I thought it proper to repeat the same experiments with pure soda prepared in a different way.

purified with and without alcohol.

I took a kilogramme [35¼ oz avoird.] of perfectly pure crystallized sulphate of soda; decomposed it by means of barytes, taking care to use a little excess; filtered; and evaporated quickly to dryness. Half of the residuum was put into alcohol, and treated as usual. The other half was dissolved in barytes water. The liquor, containing but a slight excess of barytes, was filtered, speedily evaporated, and fused in a silver crucible at a cherry red heat, as the portion prepared with alcohol had been.

The latter neutralized most acid.

Of these two specimens, 100 parts of that prepared with alcohol required 119·6 of concentrated acid for their saturation; and 100 of the other took 122·4 of acid. These results confirm the preceding; and appear to demonstrate, that the soda prepared with alcohol contains only 0·71 or 0·72 of such alkali, as in the subcarbonate and sulphate of soda is neutralized by carbonic and sulphuric acid.

Similar trials with potash gave similar results.

Similar experiments repeated in the same way, substituting for the soda caustic potash prepared with alcohol, and for the carbonate and sulphate of soda the corresponding salts with base of potash, afforded analogous results, and authorize me to conclude, that potash prepared with alcohol, far from being pure potash, contains only 0·72 or 0·73 of real alkali.

These facts require a revision of many analyses.

If these experiments be accurate, it follows, that potash and soda prepared with alcohol cannot be employed to ascertain by synthesis the proportions of the constituent principles of saline substances, that have these alkalis for their base. This is an important corollary, since it requires a revision of many experiments founded on this principle, in order to correct their results; or at least to confirm the alterations, that so great a difference in the principal datum must occasion.

Instances in Vauquelin's analysis of different sorts of potash,

Among the examples I might adduce, I shall select such as appear to me best fitted to establish the truth of this position. In the year 10 Mr. Vauquelin published an important essay on the analysis of different kinds of potash, and on the means of readily ascertaining the quantity of pure alkali in them. In this paper, which has already been so serviceable to the arts, both by its immediate application, and by giving birth to the researches of Mr. Descroizilles, the author, after having ascertained the quantity of nitric acid of a known density necessary to neutralize a given quantity of potash purified by means of alcohol, offers this result as a term of comparison, and as a standard of the greatest possible purity of potash. This however would give a very erroneous computation of the quantity of real alkali, as it appears, that the potash taken as a standard contains only 0·73 of its weight of pure potash.

and ascertaining the component parts of salts.

It is more especially in determining the proportion of the constituent principles of salts, that this source of errour is to be carefully avoided; for we know how important a good solution of this problem would be, and how great the difficulties are, that have hitherto prevented our attaining it. Mr. Berthollet, in his inquiries into the laws of affinity, applying new methods of experimenting to this question, examined those employed by Richter and Kirwan in their labours on the same subject. He found, that Kirwan, beside the number of estimations he was obliged to make, had set out with a principle of too little accuracy; and to this he ascribed much of the uncertainty of the results this chemist obtained. Yet Kirwan, by employing solutions of subcarbonate of potash and of soda, to ascertain the proportions of the salts that have these alkalis for their base, had only to apprehend the slight errour inseparable from every such analysis; and if the determination of the quantities of acid employed to saturate these carbonates had been founded on more certain data, the results of his experiments would have been much nearer the truth.

Kirwan's use of the subcarbonates less objectionable.

Berthollet employed the muriatic acid and pure alkalis,

Mr. Berthollet took a more direct method, and the nature of the muriatic acid he employed, being better ascertained, would have led him to accurate results, if the quantity of water, which the muriatic acid gas probably retains, could have been determined; and if he had taken, like Kirwan, the alkaline carbonates as the basis of his labours.

It appears to me, that the preference given to potash and soda prepared with alcohol has introduced into these delicate experiments a source of errour, which is so much the greater as it applies to the substances that predominate in the compounds, the proportions of which were to be ascertained. Mr. Berthollet establishes it as a principle, that 100 parts of potash prepared with alcohol, and kept in fusion for a quarter of an hour, require 61·5 of muriatic acid to neutralize

tralize them; and that 100 parts of soda, prepared in the same manner, take 88. From the results of the experiments above given however, we must infer, that 84·2 of muriatic acid are required, to saturate 100 parts of potash; and 120·5 of the same acid, to neutralize 100 of pure soda: whence it follows, that, the strengths of these alkalis being represented by other numbers, when they are compared with those of bases, the nature of which is fully ascertained, they must give different proportions from those mentioned in the work of Mr. Berthollet.

but the carbonates preferable.

The capacities of saturation of the carbonates, being ascertained by analysis, are liable only to little variation; and then the degree of energy of the muriatic acid approaches nearer to that of the carbonic acid, which has some influence on the results deduced from a comparison of them.

Other neutral salts.

The same reasoning applies to experiments made on the sulphates, nitrates, and phosphates, with potash or soda for their base; but I shall confine myself to a few observations on the experiments, which Mr. Berthollet has published in chap. 18 of the work I have mentioned. To find the quantity of water muriatic acid gas can retain, Mr. Berthollet neutralized 100 parts of potash prepared with alcohol, and kept some time in fusion. The muriate obtained was carefully dried, and weighed only 126·6, instead of 161·5, which ought to have been its weight. Ought not this difference, which is in some measure owing to the water contained in the muriatic acid gas, to be attributed in part to the water, or foreign matter, which forms 0·27 of the potash employed? And may we not thus account for the great differences, that exist between the numbers representing the component parts of muriate of potash in the experiments of Berthollet, Kirwan, and Richter? At least we are naturally led to presume so, from the facts I have given above.

Berthollet's examination of muriatic gas for water defective.

The addition to the alkalis probably water, but this not ascertained.

I regret the not having been able to ascertain the nature of the foreign matter, which is always found combined with soda and potash prepared by means of alcohol. I cannot venture therefore to assert any thing on the subject; but I believe, that water acts a considerable part in these phenomena; and I could have wished to have had time to examine the products, which the two alkalis thus prepared, and exposed to various degrees of heat in contact with different inflammable substances well dried, would have afforded.*

V.

The Bakerian Lecture for 1809. On some new Electrochemical Researches, on various Objects, particularly the metallic Bodies, from the Alkalis, and Earths, and on some Combinations of Hidrogen. By HUMPHRY DAVY, Esq. Sec. R.S. F.R.S.E. M.R.I.A.

(Continued from vol. XXVI, p. 339.)

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

Queries respecting nitrogen.

ONE of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia was, that nitrogen might possibly consist of oxigen and hidrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same path of inquiry, and which at first view do not appear unpromising.

Formation of Nitrogen in various processes errone-

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into ously asserted.

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* Mr. Gay-Lussac, in his report of this paper to the Institute, observed, that Mr. Berthollet, in some experiments which at that time he had communicated only to a few friends, had already found, that potash prepared with alcohol contained at least 0·13 of water, after being exposed to a red heat.

into the discussion of the ideas entertained by the German chemists on the origin of nitrogen, produced during the passage of water through redhot tubes, or the speculations of Girtanner, founded on these and other erroneous data; the early discovery of Priestley on the passage of gasses through redhot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon Lagrange, have afforded a complete solution of this problem.

Nitrogen supposed to appear in the decomposition and recomposition of water by electricity. Experiment for proof of this

One of the most striking cases in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity*. To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxigen and hidrogen with great rapidity, and in which these gasses could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gasses, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about half a cubic inch: it had been carefully purged of air by the airpump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere while warm. After the first detonation of the oxigen and hidrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{10}$ of the volume of the gasses, after every detonation this residuum was found to increase; and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, i. e. $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas;

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* See Dr. Pearson's elaborate experiments on the decomposition of water by electrical explosions. Nicholson's Journal, 4to, vol. I, p. 301.

It contained no oxigen; 6 measures mixed with 3 measures of oxigen diminished to 5·1, so that it consisted of 2·6 of hidrogen, and 3·4 of a gas, having the characters of nitrogen.

apparently in favour of it.

But the nitrogen probably from the atmosphere.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible, that, at the moment of the explosion by the electrical discharge, the sudden expansions and contractions, might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method, by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the preceding one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{34}{100}$ of a cubical inch. It was carefully examined; six measures of it detonated with three measures of oxigen, diminished to rather less than 1 measure. A result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hidrogen. That the hidrogen is in excess may be easily referred to a slight oxidation of the platina.

In the production of water no nitrous acid produced unless nitrogen present.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxigen, hidrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid, sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxigen and hidrogen, and is never produced from those two gasses alone. In the Bakerian lecture for 1806, I have stated several facts, which seemed to show, that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed unless *nitrogen* be present.

Experiments to prove, that no acid or alkali is produced from pure water.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find, that the assertion, that both acids and alkalis may be produced from pure water, has again been repeated*. The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test, than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hidrogen gas, that no nitrous acid or alkali was formed. It might be said, that in this case the presence of hidrogen dissolved in water would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxigen gas, and the other in an apparatus, in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gasses were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases, for two or three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxigen, from oximuriate of potash, was any acid or alkali exhibited in the cones: even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though if the asbestus was touched with unwashed hands, or the smallest particle of neutrosaline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

1st series of experiments.

No acid or alkali appeared,

except when nitrogen is present, or the conductor touched with unwiped hands.

In the second series of experiments, the oxigen and hidrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found, that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which

2d. series of experiments.

Alkali in the negative glass, acid in the positive.

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* Nicholson's Journal, August, 1809, p. 258.

could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities, which might rise in distillation with the mercury, or to muriatic

Muriatic acid in glass.

acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely, that muriatic acid in its acid state may exist in combination in glass.

Platina ignited in oxigen gas and aqueous vapour.

I tried several experiments on the ignition and fusion of platina, by Voltaic electricity, in mixtures of the vapour of water and oxigen gas. I thought it possible, if water could be combined with *more oxigen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxigen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oximuriate of potash, there was not the slightest indication of such a result.

Aqueous vapour passed through red-hot oxide of manganese formed nitrous acid

Water in vapour was passed through oxide of manganese, made redhot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper was formed.

uniformly in a large tube.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid however was generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

Attempt to produce ammonia from charcoal and pearlash by the action of water.

I stated in the last Bakerian Lecture, that, in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited, by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hidrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant, as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner, that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it; figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

Ammonia produced from the same mixture 3 or 4 times,

In all cases, in which the water was brought into contact with the mixture of charcoal and potash when it was perfectly cool, and afterwards distilled from it by a slow heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

and again on the addition of fresh potash.

More ammonia produced, when the mixture is cooled in contact with the atmosphere.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

Perhaps no nitrogen composed in this process.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of Mr. Berthollet, that nitrogen adheres very strongly to charcoal*.

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* Mem d'Arcueil, Tom. II, page 485.

The circumstances, that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea, that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

Nitrogen produced during the freezing of water.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury while hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing, there was no notable increase of the volume, At the end of the experiment about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice, and thawed four successive times in a conical vessel of wrought iron. At the end of the fourth process the volume of the gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxigen, $\frac{3}{10}$ hidrogen. and $\frac{6}{10}$ nitrogen.

Nitrous gas and sulphuretted hidrogen kept in contact, diminish much.

Mr. Kirwan observed the fact, that, when nitrous gas and sulphuretted hidrogen are kept in contact for some time, there is a great diminution of volume; and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gasses, when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

Reasonings on this.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hidrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail toward the conclusion of this lecture, contains a volume of hidrogen equal to its own. But one of hidrogen demands half its volume of oxigen to convert it into water, and nitrous gas consists of about half a part in volume of oxigen; so that, supposing the whole of the hidrogen employed in absorbing oxigen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hidrogen over mercury, the barometer being at 29·5$^{\text{in.}}$, thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gasses quickly diminished; after two days the diminution ceased; and the volume became stationary; the barometer was at 30·45$^{\text{in.}}$, and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

Experiment.

Results.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds: nitrogen, hidrogen, oxigen, and sulphur combining to form one; and a part of the nitrogen and oxigen becoming more condensed, to form another.

Attempts to decompose nitrogen.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompose nitrogen, by agents which I conceived might act at the same time on oxigen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed; but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

Experiment. I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant; as soon as the contact with the potassium was made, there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxigen gas.

Results. In all trials of this kind hidrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference, that nitrogen is decompounded in the process; but I found, that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hidrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen, which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hidrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely, that the slight loss is owing to its combination with nascent hidrogen,

gen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced, when potassium is electrised and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hidrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

Experiment with phosphuret of lime.

As almost all compounds known to contain hidrogen are readily decomposed by oximuriatic acid gas, a mixture of nitrogen and oximuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water; there was a small loss of nitrogen; but the greatest part came over densely clouded, and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

A mixture of nitrogen and oximuriatic acid gas exposed to heat.

The general tenour of these inquiries cannot be considered as strengthening in any considerable degree the suspicion, which I formed of the decomposition of nitrogen, by the distillation of the olive coloured substance from potassium and ammonia in tubes of iron.

No confirmation of the decomposition of nitrogen

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming, that it has been converted into new matter. Though the iron tubes, which I used, were carefully cleaned; yet still it was not unlikely, that a small quantity of oxide might adhere to the welded parts; the oxigen of which, in the beginning of the process of distillation, might form water with hidrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would be again brought into action toward the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hidrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube

The loss of nitrogen accounted for.

Experiment to ascertain this.

tube used immediately after the whole internal surface had been cleaned by the borer. Six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hidrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly 1 cubical inch of ammonia, 4 of nitrogen, and 8 of hidrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hidrogen, and 1·1 nitrogen. The potassium lost in the operation was sufficient to generate 3·1 cubical inches of hidrogen.

Part of the potassium united with the iron.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of Mr. Hassenfratz*, that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which I conceive could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hidrogen were found to be generated.

Apparent loss of nitrogen accounted for.

In the experiments detailed in page 53 of the last volume of the Transactions †, a loss of nitrogen, and a production of hidrogen were perceived in a case, in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible, that ammonia might have been regenerated by hidrogen from the naphtha, and absorbed by that fluid; and a part of the hidrogen might likewise proceed from the decomposition of the naphtha; and in several experiments, in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

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* Journal des Mines, April, 1808, p. 275. See Journal, vol. XXV, p. 51

† Journal, vol. XXIII, p. 252, 253.

Even the considerable excess of hidrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451 *, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

But the question still doubtful.

These objections are the strongest that occur to me, against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Farther experiments on the decomposition of ammonia.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some farther operations, which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxigen was probably separated in this operation; but I was aware, that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxigen in this substance; at the same time it led to the suspicion, that of the two gasses separated by electricity one, or perhaps both, might contain metallic matter united to oxigen: and the results of the distillation of the fusible substance from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

Method of conducting them.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an appa-

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* Journal, vol. XXV, p. 137.

ratus (of which a figure is attached to this paper,) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock, which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

Reason of keeping the volume of gas the same.

The object in keeping the volume the same during the decomposition was to produce the condensation of any aqueous vapour, which, if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gasses,) might, in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hidrogen and nitrogen, as in the ammonia.

Results.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization, but the wires were uniformly tarnished, and in an experiment in which surfaces of brass were used, a small quantity of olive coloured matter formed on the metal, but though in this case nearly 8 cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to —15°, there was a very feeble indication given of the addition of hygrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hidrogen was to the nitrogen in the average proportions of from 73 or 74 to 27 or 26; the proper corrections being made, and the precautions before referred to being taken*.

Assuming

Berthollet's experiments on the decomposition of ammonia

* Philosophical Transactions, 1809, page 459 [Journal, vol XXV, p. 143, 144] Mr Berthollet, jun., in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of [illegible] of the oxigen, separated in

Specific gravities of the gasses examined.

Assuming the common estimations of the specific gravity of ammonia, of hidrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807 would be supported by these new experiments, but as the moisture and oxigen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia; I resolved to investigate more precisely, than I had reason to think had been hitherto done, the specific gravities of the gasses concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power.

Sp grav. of nitrogen, hidrogen, and ammonia.

Nitrogen, hidrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be, at 30·5in. barometer, 51° Fahrenheit's thermometer,

For nitrogen, the 100 cubical inches	29·8	grains.
For hidrogen, ditto	2·27	
For ammonia	18·4	

Loss in the decomposition of ammonia.

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gasses evolved; there is a loss

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in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent and at the same time he confutes some experiments, which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxigen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind It is possible, that the volume may be exactly doubled, and that the nitrogen may be to the hidrogen as one to three; but, neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

of $\frac{1}{18}$*, and if the smallest proportion be taken the loss will be nearly $\frac{1}{13}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

Probably ammonia composed of hidrogen & nitrogen only.

It is *possible*, that the minute quantity of oxigen, which appears to be separated, is not accidental, but a result of the decomposition; and if hidrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hidrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

What is the metallic basis of the volatile alkali?

But if *ammonia* be capable of decomposition into nitrogen and hidrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions, intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

Water always adheres to it.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices, with the hopes of being able to form it from ammonia in a dry state, but without success. Neither of the amalgams of potassium, sodium, or barium, produces it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

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* 100 of ammonia, at the rate of 185, will give 136·9 of hidrogen, weighing 3·1 grains, and 48·1 of nitrogen weighing 14·33 grains; but 18·4 — 17·4 = 1: and at the rate of 180, 133 of hidrogen weighing 3·4, and 47 of nitrogen, weighing 14; and 18·4 — 17 = 1·4.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct, than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

Driest amalgam obtained.

Decomposes very slowly.

Attempts to distil ammonium from it.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hidrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{1}{10}$ to $\frac{6}{10}$ of hidrogen produced.

Ammonia, if an oxide, contains 48 per cent oxigen.

As it does not seem possible to obtain an amalgam in a uniform state as to adhering moisture, it is not easy to say what would be the exact ratio between the hidrogen and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide,) must contain about 48 per cent of oxigen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases*.

If hidrogen be a simple substance, nitrogen contains 48 oxigen, 34 basis.

If hidrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxigen, and 34 of basis; but if the opinion be adopted, that hidrogen and nitrogen are both oxides of the same metal, then the quantity of oxigen in nitrogen must be supposed less.

Phlogistic hypothesis.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis of the nature of metallic substances, but, if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hidrogen, and metallic by combining with a greater proportion.

Proportions of the amalgam.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter, as it is conceived to contain on the hypothesis of deoxigenation. In the last Bakerian lecture I have rated the proportion at $\frac{1}{12000}$, but this is the least quantity that can be assumed, the mercury being supposed to give off only once and a half its volume of ammonia. If the proportion stated in page 53 be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1000}$ of new matter, on the antiphlogistic view, and about $\frac{1}{560}$ on the phlogistic view.

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* Even in common air, the amalgam evolves hidrogen and ammonia, nearly in these proportions, and in one experiment, which I lately tried, there seemed to be no absorption of oxigen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali, for if the hidrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that, being in the nascent state, it would rapidly absorb oxigen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxigen had been absorbed, but this difference might have arisen, partly at least, from the mixture of hidrogen. Whether in any case the amalgam absorbs oxigen gas, is a question for farther investigation.

I shall have occasion to recur to, and to discuss more fully these ideas, and I shall conclude this section by stating, that, though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe, that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

New modes of manufacturing nitrous acid and volatile alkali suggested.

(To be concluded in our next.)

VI.

Times of Migration of some of the Swallow Tribe, &c., near London. In a Letter from THOMAS FORSTER, *Esq.*

To Mr. NICHOLSON.

SIR,

SHOULD you consider the following table, showing the periods of the earliest and latest appearance of several of the swallow tribe, &c., at Clapton, during some years, worth insertion in your Journal, it is much at your service. It may amuse some of your numerous readers, and will oblige your constant reader,

THOMAS FORSTER.

EARLIEST

Time of certain birds appearing and disappearing.

	EARLIEST APPEARANCE.					LATEST.	
	1806	1807	1808	1809	1810	1808	1809
Hirundo rustica, Common swallow.	Apr. 2	May 1	Apr. 18	Apr. 28	Apr. 21	Oct. 17	Oct. 3
Hirundo urbica, Martin.	Apr. 26	May 1	May 1	May 5	Apr. 21	Oct. 18	Oct. 16
Hirundo apus, Swift.		May 16	May 14		May 19	Aug. 14	Aug. 13
Jynx, or yunx torquilla, Wryneck.	May 1	Apr. 30	May 1		Apr. 21		

VII.

The Croonian Lecture. On the Functions of the Heart and Arteries. By THOMAS YOUNG, *M. D. For. Sec. R. S.*[*]

Mechanical motion in a living body subject to the laws of dead matter:

THE mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye; and there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process, calculated to overcome those affections by others, which are commensurate to them, and which are of a contrary tendency. For example,

but the vital powers can institute processes to counteract different affections.

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[*] Philos. Trans. for 1809, p. 1.

example, animal bodies are incapable of being frozen by a considerable degree of cold, because animals have the power of generating heat; but the skin of an animal has no power of generating an acid, or an alkali, to neutralize the action of an alkaline or an acid caustic, and therefore its texture is destroyed by the chemical attraction of such an agent, when it comes into contact with it. As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies; these laws being fully adequate to the explanation of the connection between the motive powers, which are employed in the system, and the immediate effects, which they are capable of producing, in the solids or fluids of the body: and it is obvious, that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of these powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics.

As far as the functions depend on motion they obey mechanical laws,

the circulation therefore an object of hydraulics.

Inquiries into the functions of the heart and arteries

In examining the functions of the heart and arteries, I shall inquire, in the first place, upon the ground of the hydraulic investigations which I have already submitted to the Royal Society*, what would be the nature of the circulation of the blood, if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone; in the second place, in what manner the pulse would be transmitted from the heart through the arteries, if they were merely elastic tubes, and in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall lastly add some observations on the disturbances of these motions, which may be supposed to occur in different kinds of inflammations and fevers.

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* See Journal, vol. XXII, p. 104. The reader is requested to substitute in p. 121, l. 5 from bot., for $m\sqrt{\frac{a}{1}}$, $m\sqrt{\frac{a}{2}}$; in p. 123, l. 8 and 9 from bot., for when c c d, whence d; and in p. 122, at the end of l. 6 from bot., to add—is denoted by ac.

When

The blood vessels considered as tubes of invariable dimensions.

When we consider the blood vessels as tubes of invariable dimensions, we may suppose, in order to determine the velocity of the blood in their different parts, and the resistances opposed to its motion, that this motion is nearly uniform, since the alterations arising from the pulsation of the heart do not materially affect the calculation, especially as they are much less sensible in the smaller vessels than in the larger ones, and the principal part of the resistance arises from these small vessels. We are to consider the blood in the arteries as subjected to a certain pressure, by means of which it is forced into the veins, where the tension is much less considerable, and this pressure, originating from the contractions of the heart and continued by the tension of the arteries, is almost entirely employed in overcoming the friction of the vessels: for the force required to overcome the inertia of the blood is so inconsiderable, that it may, without impropriety, be wholly neglected. We must therefore inquire, what the magnitude of this pressure is, and what degree of resistance we can suppose to arise from the friction of the internal surface of the blood vessels, or from any other causes of retardation. The magnitude of the pressure has been ascertained by Hales's most interesting experiments on a variety of animals, and may thence be estimated with sufficient accuracy for the human body; and for determining the magnitude of the resistance, I shall employ the theorems which I have deduced from my own experiments on very minute tubes, compared with those which had been made by former observers under different circumstances; together with some comparative experiments on the motion of water and of other fluids in the same tubes.

Force with which the blood is propelled from the arteries into the veins.

Dr. Hales infers, from his experiments on quadrupeds of different sizes, that the blood in the human arteries is subjected to a pressure, which is measured by a column of the height of seven feet and a half: in the veins, on the contrary, the pressure appears to amount to about six inches only: so that the force, which urges the blood from the greater arteries through the minuter vessels into the large veins, may be considered as equivalent to the pressure of a column of seven feet.

Calculation of the resistance.

In order to calculate the magnitude of the resistance, it is necessary to determine the dimensions of the arterial system, and the velocity of the blood which flows through it. According to the measurements of Keill and others, we may take $\frac{3}{4}$ of an inch for the usual diameter of the aorta, and suppose each arterial trunk to be divided into two branches, the diameter of each being about $\frac{4}{5}$ of that of the trunk, (or more accurately, $1 : 1{\cdot}26 = 10^{-{\cdot}100367}$), and the joint areas of the sections about a fourth part greater, for $1{\cdot}2586 : 1 = 10^{{\cdot}099894}$). This division must be continued twenty-nine times, so that the diameter of the thirtieth segment may be only the eleven hundredth part of an inch, that is, nearly large enough to admit two globules of the blood to pass at once. The length of the first segment must be assumed about nine inches, that of the last, the twentieth of an inch only; and supposing the lengths of the intermediate segments to be a series of mean proportionals, each of them must be about one sixth part shorter than the preceding, (or $1 \; 1{\cdot}961 = 10^{-{\cdot}07776}$), the mean length of the whole forty-six inches, the capacity to that of the first segment as 72·71 to 1, and consequently the weight of the blood contained in the arterial system about 9·7 pounds. It is probable that this calculation approaches sufficiently near to the truth: for the whole quantity of blood in the body being about 40 pounds, although some have supposed it only 20, others no less than 100, there is reason to believe, that half of this quantity is contained in the veins of the general circulation, and that the other half is divided, nearly in equal proportions, between the pulmonary system and the remaining arteries of the body, so that the arteries of the general circulation may contain about 9 or 10 pounds. Haller allows 50 pounds of circulating fluid, partly serous, and partly red, and supposes $\frac{1}{6}$ of this to be contained in all the arteries taken together: but in a determination which must be in great measure conjectural we cannot expect perfect accuracy: and according to Haller's own account of the proportions of the sections of the arteries and veins, the large trunks of the veins appear to be little more than twice as capacious as those of the arteries, and the smaller branches much more nearly equal, so that

Quantity of blood in the system.

that we cannot attribute to the arterial system less than $\frac{1}{4}$ of the whole blood.

An ounce and half thrown out at each pulsation of the heart.

Velocity from $8\frac{1}{2}$ in. to a 93d of an inch in a second.

Haller questioned the accuracy of Hales,

but he sometimes reasoned erroneously,

Resistance from friction if the blood were water.

It may be supposed that the heart throws out, at each pulsation, that is about seventy-five times in a minute, an ounce and a half of blood: hence the mean velocity in the aorta becomes eight inches and a half in a second: and the velocity in each of the succeeding segments must of course be smaller, in proportion as the joint areas of all the corresponding sections are larger than the area of the aorta: for example, in the last order of vessels, of which the diameter is the eleven hundredth of an inch, the velocity will be one ninety-third of an inch: and this result agrees sufficiently well with Hales's observation of the velocity in the capillary arteries of a frog, which was one ninetieth part of an inch only. It is true that Haller is disposed to question the accuracy of this observation, and to attribute a much greater velocity to the blood flowing through the capillary vessels, but he did not attempt either to measure the velocity, or to determine it by calculation: nor is this the only instance in which Haller has been led to reason erroneously, from a want of mathematical knowledge: he may, however, have observed the particles of blood moving in the axis of a vessel with a velocity much exceeding the mean velocity of its whole contents. If we calculate upon these foundations, from the formula which I have already laid before the Society, it will appear, that the resistance which the friction of the arteries would occasion, if water circulated in them instead of blood, with an equal velocity, must amount to a force equivalent to the pressure of a column of fifteen inches and a half: to this we may add about a fourth for the resistance of the capillary veins, and we may estimate the whole fricton for water, at twenty inches. The only considerable part of this force is derived from the term $\frac{1\cdot926lv}{40d^{3\cdot5}}$ in the value of f: the term increases for each successive segment in the ratio $1 : 1\cdot49425 = 1 : n$, and the sum of the series is to the first term, as $\frac{n^{30}-1}{n-1}$ to 1.

The resi tance very little ex-

It appears also, that a very small portion only of the resistance is created in the larger vessels: thus as far as the twentieth cept in the minute vessels.

twentieth division, at the distance of an inch and a quarter only from the extreme capillary arteries, the pressure of a column of one twentieth of an inch only is required for overcoming the whole friction; and at the twenty-fifth division, where the artery does not much exceed the diameter of a human hair, the height to which the water would rise, in a tube fixed laterally into the artery, is only two inches less than in the immediate neighbourhood of the heart.

Resistance of fluids more or less viscid.

In order to judge of the comparative resistance produced by fluids of different degrees of viscidity, I employed the same tubes, by means of which I had determined the friction of water, in extreme cases, for ascertaining the effect of different substances held in solution in the water: since it is impossible to make direct experiments on the blood in its natural state, on account of its tendency to coagulate; and those substances, which have the power of preventing its coagulation, may naturally be supposed to produce a material change in its viscidity. The diameter of one of the tubes, which was cylindrical, was the fortieth part of an inch: the bore of the other was oval, as is usual in the finest tubes made for thermometers: the section, divided by one fourth of the circumference, gave one hundred and seventy seconds for the mean diameter. I caused some milk, and solutions of sugar of different strength, to pass through these tubes: they were all transmitted much more sparingly than water, with an equal pressure, and the difference was more considerable in the smaller than in the larger tube, as might naturally be expected, both from the nature of the resistance, and from the result of Gerstner's experiments on water at different temperatures. In the first tube the resistance to the motion of milk was three times as great as to that of water, a solution of sugar in five times its weight of water produced twice as much resistance as water; in twice its weight, nearly four times as much as water: but in the narrower tube, the weaker solution of sugar exhibited a resistance five times as great as that of water, which is more than twice as much as appeared in the larger tube. Hence there can be no doubt, that the resistance of the internal surface of the arteries to the motion of the blood must be much greater, than would be found

Calculation for blood.

in the case of water; and supposing it about four times as great, instead of 20 inches, we shall have 80, for the measure of a column of which the pressure is capable of forcing the blood, in its natural course, through the smaller arteries and veins, which agrees very well with Hales's estimate.

The calculation founded on preceding observations.

This determination of the probable dimensions of the arterial system, and of the resistances occasioned by its different parts, is in some few respects arbitrary; at the same time that it cannot be materially altered, without altering either the whole quantity of blood contained in the body, the diameters of the smallest capillary vessels, the mean number of bifurcations, or the magnitude of the resistance, all of which are here assumed nearly as they have been laid down by former observers; the estimation of the length of the successive segments only is made in such a manner, as to reconcile these data with each other, by means of the experiments and calculations relating to the friction of fluids in pipes.

The curvature of the vessel increases the resistance very little.

The effect of curvature in increasing the resistance has been hitherto neglected; it can be sensible only in the larger vessels; and supposing the flexures of these to be equivalent to the circumferences of two circles, each two inches in diameter, the radius q being 1, we have $f = \frac{\cdot0000045 p v^2 q^{\frac{1}{2}}}{q} = \cdot0000045 \times 720 \times 64 = \cdot207$, or about one fifth of an inch, for the additional resistance arising from this cause in the case of water, or four fifths for blood, which is a very inconsiderable part of the whole.

Objections to the experiments answered.

It might be questioned whether the experiments, which I have made, with tubes $\frac{1}{173}$ of an inch in diameter, are sufficient for determining, with accuracy, the degree in which the resistance would be increased in tubes, of which the diameter is only one sixth part as great; and it may be doubted whether the analogy, derived from these experiments, can be safely employed as a ground for asserting, that so large a portion of the arterial pressure is employed in overcoming the resistance of the very minute arteries. But it must be remembered, that these experiments are at least conclusive with respect to the arteries larger than the tube employed in them, and even those which are a little smaller, so that the remaining pressure, as observed in experiments, can only be employed in overcoming the resistance of the minuter arteries and veins, and these observations tend therefore immediately to confirm the analogy drawn from the experiments on the motion of water. It might indeed be assumed, that the viscidity of the blood exceeds that of water in a much greater ratio than that which is here assigned; but this is rendered improbable by some experiments of Hales, in which, when the intestines were laid open, on the side opposite to, the mesentery, so that many of the smaller arteries were divided, the quantity of warm water which passed through them with an equal pressure, was only about twelve times as great as that of the blood which flows through them in their natural state; and it is probable, that at least three or four times as much of any fluid must have passed through them in their divided, as in their entire state, unless we suppose that the coats of the divided vessels, like many other muscular parts, are capable of being contracted by the contact of water. In some other experiments it was found, that a moderate degree of pressure was capable of causing water to exude so copiously through the exhalant vessels of the intestines, that it passed through the aorta with a velocity of about two inches in a second, although these vessels do not naturally allow any passage to the blood: on the other hand, it sometimes happened, that very little water would pass through such channels as naturally transmitted a much larger quantity of blood: a circumstance which Dr. Hales very judiciously attributes to the oozing of the water into the cellular membrane surrounding the vessels, by means of which they were compressed, and their diameters lessened. On the whole, it is not improbable, that in some cases the resistance, opposed to the motion of the blood, may exceed that of water in a ratio somewhat greater than I have assigned; but this must be in the minutest of the vessels, whilst in the larger arteries the disproportion must be less: so that, however we may view the subject, it appears to be established, that the only considerable resistance, which the blood experiences, occurs in the extreme capillary arteries, of which the diameter scarcely exceeds the hundredth part of an inch.

We cannot suppose, that the dimensions of the sanguife- The truth of

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The inference not affected by the variations that may be supposed in the dimensions.

Size of the globules of the blood.

rous system agree uniformly, in all its parts, with the measures which I have laid down; but the truth of the inference is not affected by these variations. For example, there may perhaps be some arteries communicating with veins, of which the diameter exceeds the eleven hundredths of an inch; but there are certainly many others, which are much more minute; and the blood, or its more liquid parts, passing through these more slowly, it must move more rapidly in the former, so that the resistance may in all be equal to the pressure, and the mean velocity may still remain such as is determined by the quantity of blood passing through the aorta. There is indeed some uncertainty in the measure of the globules of the blood, which I have made the basis of the dimensions of the minute arteries: and I have reason to think, that instead of $\frac{1}{3000}$ of an inch, their greatest diameter does not exceed $\frac{1}{3500}$, or even $\frac{1}{3600}$: the general results of the investigation are not however affected by this difference: it will only require us to suppose the subdivisions somewhat more numerous, and the branches shorter.

Nature and velocity of the propagation of the pulse.

Elasticity of the coats of the arteries.

These are the principal circumstances, which require to be considered, with respect to the simple transmission of the blood through the arteries into the veins, without regard to the alternate motion of the heart, and to the elastic and muscular powers of the vessels. I shall next examine the nature and velocity of the propagation of the pulse. The successive transmission of the pulsations of the heart, through the length of the arteries, is so analogous to the motion of the waves on the surface of water, or to that of a sound transmitted through the air, that the same calculations will serve for determining the principal affections of all these kinds of motion; and if the water, which is agitated by waves, is supposed to flow at the same time in a continued stream, and the air which conveys a sound to be carried forwards also in the form of a wind, the similitude will be still stronger. The coats of the arteries may perhaps be considered, without much inaccuracy, as perfectly elastic; that is, as producing a force proportional to the degree in which they are extended beyond their natural dimensions; but it is not impossible, that there may be some bodies in nature, which differ materially from this general law, especially where the distension becomes considerable: thus there may be substances, which exhibit a force of tension proportional to the excess of the square, or the cube of their length, beyond a certain given quantity. It is safest therefore to reason upon the elasticity of any substance, from experiments made without any great deviation from the circumstances to which the calculation is to be applied.

The law of elasticity may vary.

For this purpose, we may again employ some of the many excellent experiments contained in Hales's hæmastatics. It appears, that, when any small alteration was made in the quantity of blood contained in the arteries of an animal, the height of the column, which measured the pressure, was altered nearly in the same proportion, as far as we are capable of estimating the quantity, which was probably contained in the larger vessels of the animal. Hence it follows, that the velocity of the pulse must be nearly the same as that of an impulse transmitted through an elastic fluid, under the pressure of a column of the same height, as that which measures the actual arterial pressure: that is, equal to that which is acquired by a heavy body falling freely through half this height. In man, this velocity becomes about fifteen feet and a half in a second; to which the progressive motion of the blood itself adds about eight inches; and with this velocity, of at least sixteen feet in a second, it may easily happen, that the pulse may appear to arrive at the most distant parts of the body without the intervention of any very perceptible interval of time.

Velocity of the transmission of the pulse.

The velocity of the transmission of the pulse being known, it is easy to determine the degree in which the arteries are dilated during its passage through them. The mean velocity of the blood in the aorta being eight inches and a half in a second, its greatest velocity must be about three times as much, since the contraction of the heart is supposed to occupy only about one third part of the interval between two successive pulsations; and if the velocity of the pulse is sixteen feet in a second, that of the blood itself must be about one eighth part as great; so that the column of blood occupying eight inches may occupy only seven; hence the diameter must increase in the ratio of

Degree of dilatation of the arteries.

about fifteen to sixteen. The tension will also become one eighth greater, and the force of the heart must be capable of supporting a column of one hundred and one inches. This force would, however, require to be somewhat increased, from the consideration, that the force required at the end of any canal, during the reflection of a pulsation or wave of any kind, is twice as great as the force exerted during its transmission; and the force employed in the origination of a wave or pulse in a quiescent fluid is the same as is required for its reflection; on the other hand, a weaker pulsation, proceeding into a narrower channel, becomes more energetic, so that, from this consideration, a force somewhat smaller would be required in the heart: on the whole, however, it appears probable, that the former of these corrections must be the more considerable, and that the force of the heart must be measured by the pressure of a column rather more than less than one hundred and one inches high: nor would this force by any means require a strong exertion of muscular power; for it only implies a tension of something less than three pounds for each inch of the circumference of the greatest section of the heart; and supposing the mean thickness half an inch, an equal number of the fibres of some other muscles of the body would be capable of exerting a force of more than two hundred pounds, in the state of the greatest possible action.

The force agrees with an experiment of Hales.

The force, here assigned to each pulsation, agrees extremely well with the inference, that may be drawn from an experiment of Hales, on the ascent of the blood in a tube connected with an artery of a horse. The whole height of the column being nine feet, the blood rose about three inches higher during each pulsation, which was repeated fifty or sixty times in a minute: now we may suppose the acceleration to have extended a little beyond the first half of the space thus described, so that two inches were described in two fifths of a second; and if there had been no friction, nor any other cause of retardation, there can be no doubt, that at least four inches would have been described in the same time; but the same column of nine feet, if it had been actuated by its own weight, would have described thirty one inches in the same time: consequently the force, with which the blood was forced through the artery, was nearly one eighth of the whole force of tension, as it appears in the former calculation.

Diminution of the pulse in the smaller arteries.

The magnitude of the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of the joint areas, in the same manner as the intensity of sound is shown to decrease in diverging from a centre, in the subduplicate ratio of the quantity of matter affected by its motion at the same time. For example, in the arteries of the tenth order, of which the diameter is one thirteenth of an inch, its magnitude must be only one third as great as in the aorta, that is, the greatest progressive velocity of the blood must be eight inches and a half in a second only, and the dilatation one fiftieth part only of the diameter. In the vessels of the twentieth order, the dilatation does not exceed $\frac{1}{100}$ of the diameter, which is itself the 140th part only of an inch: so that it is not surprising, that Haller should have been unable to discover any dilatation in vessels of these dimensions, even with the assistance of a powerful microscope. If we estimated the magnitude of the pulse in the aorta, from the excess of the temporary above the mean velocity, which would perhaps be justifiable, that magnitude would be still less considerable.

Velocity of the pul the larger arteries more considerable than here stated.

These calculations agree extremely well with each other, and with experiment, as far as they relate to the power of the heart, and the affections of the smaller arteries. But there is reason to think, that the velocity of the pulse the larger vessels is much more considerable, than has been here stated; and their dilatation is also less conspicuous, when they are exposed to view, than it would probably be, if it were as great as is inferred from the velocity here assigned. I have demonstrated in the hydraulic investigations which I lately laid before the Royal Society, that the velocity of an impulse passing through a tube, consisting of perfectly elastic materials, is half as great as that of a body supposed to have fallen from the given point to the base of the modular column of the tube; and that the height of this column is such, that the tube would be extended without limit by its pressure: consequently it must be greater than the height of a column equivalent to the pressure

by

Pressure required to burst the carotids of a dog.

by which the tube is burst. Now it has been ascertained by Dr. Hales, that the pressure, required for bursting one of the carotids of a dog, is equal to that of a column of water one hundred and ninety feet high; nor does he remark, that the artery was very materially dilated; and deducting from this height the five feet, which express the actual pressure in the arteries of a dog, the remaining one hundred and eighty five feet will give a velocity of at least fifty four feet in a second, for the propagation of the pulse in the dog. It is not however ascertained, that all the membranes, which may have surrounded the artery in this experiment, are called into action in its ordinary pulsation; much less that the force, developed by their tension, varies precisely according to the general law of perfectly elastic bodies; but this mode of calculation is still amply sufficient to make it probable, that the velocity of the pulsations, in the larger arteries, must amount to at least forty feet in a second, although some very considerable deductions must be made, on account of the resistances of various kinds, which cannot be comprehended in the calculation.

The artery does not subside to its former dimensions immediately.

The artery must not be supposed to subside, immediately after each pulsation, precisely to its original dimensions; since it must remain somewhat fuller, in order to supply the capillary arteries, and the veins, in the interval between the two successive pulsations: and in this respect it differs from the motions of a wave through a canal, which is open on both sides: but the difference may be understood, by supposing a partial reflection of the pulse to take place at every point where it meets with any resistance, which will leave a general distension of the artery, without any appearance of a retrogade pulsation.

(To be concluded in our next.)

VIII.

Letters from Dr. WILLIAM ROXBURGH, *of Calcutta, to Dr.* C. TAYLOR, *Secretary to the Society of Arts, &c., on various Natural Productions of the East Indies**.

MY DEAR SIR,

IT will give you pleasure to learn, that I and my family arrived at Bengal in very good health. I have not had much time to prepare any kind of communication for the Society, but shall not be idle. I trust that I shall by early conveyance receive your letter from Dr. Hunter, the Secretary of our Asiatic Society, to accompany the copy of the Transactions of the Society of Arts. Tell me what is thought of the extract of the gaub, or tannin, I sent you prepared from the fruit of diospyros glutinosus, or rather embryopteris glutinifera, *Coromandel Plants, Vol. I, No.* 70; you know you were only just put into a way of getting it from the India house, when I left you.

Extract of gaub or tannin.

I propose to get Mr. Cowper, the surgeon of the ship we came out in, to carry this, and a sample of the fibres of No. 3 of my last communication, on the Comparative Strength &c. of the Plant called Calooee by the Malays, see Vol. 24, page 148†. I can cultivate this plant to any extent, as it grows readily from slips and cuttings, is perennial, and yields three or four cuttings, or crops, annually; but the cleaning of the fibres from the glutinous fleshy exterior coat, with which they are covered and intermixed, is uncommonly difficult. It has been simply scraped off in the sample I now send you, which I think you may present to the Society, though I fear this letter is written in too great a hurry for their attention. I beg of you to try to procure me all the information you can relative to cleaning such fibres. When the shoots are cut, the bark peels off most readily, but no

Fibres of a shrubby species of nettle.

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* Trans. of the Society of Arts, vols. XXVI and XXVII.

† See Journal, vol. XVI, p. 226, 228.

kind of washing, coction, or maceration, that I have yet been able to think of, is of any use in cleaning or freeing the fibres from the exterior coat; the best way I have yet tried is scraping off this coat, as they do the pulpy part of the wild plantain, or abaca at Manilla; see Annals of Botany, vol. 1, p. 200; but such a process will, I fear, be too expensive for calooee hemp, though I know it is much stronger than any thing of the kind I could ever procure from the plantain tree indeed next to *jeetee;* this fibre is the strongest vegetable fibre known to me.

Hemp from it. I have put up two small samples of the calooee hemp. No. 1 is prepared as before mentioned, by scraping off the exterior coat as soon as the bark is pulled off. This has been cut and cleaned within these two weeks. No. 2 is the bark peeled off and dried in that state, and is about one year old, consequently done while I was in England. No. 1 seems to me to be as clean as the generality of Russian hemp. Pray let Lord Dundonald see this substance, and make my best respects to his lordship when you see him; he may be able to advise me how to proceed in cleaning it in the first instance.

Orange dye. Remember me to Dr. Bancroft, and tell him I have not forgot the orange dye, wassuntagonda, a powder procured from the outside of the capsules of my rottleria tinctoria; I must procure it from a distant country.

Gum kuteera. I have been this instant looking over the twenty-first Volume of your Society's Transactions, and think it may be agreeable to you to know, that the tree which yields the *gum kuteera,* page 423, is my STERCULIA URENS. *(See Coromandel Plants, Vol. I, No. 24.)*

I am, &c.

W. ROXBURGH.

Calcutta, Sept. 20, 1807.

My Dear Sir,

Since I wrote to you, on the 20th of September, by the surgeon of the Baring, who carried for you samples of the

Malay hemp, called calooee, I have received your letter of the 7th of March, intended to overtake me at Portsmouth, and I thank you for the pains you have taken about my myrobalan galls. If the value of them is, by this experiment, ascertained, I shall the less regret the great loss I have sustained by them. You will be able to learn this from Mr. Desanges, and let me know.

Myrobalan galls.

You have now learned how to get a treasury order for any thing I may send the Society, I shall therefore be encouraged to trouble you oftener, and just now with four pounds and a half of the extract of gaub fruit, (EMBRYOPTERIS GLUTINIFERA, *Coromandel Plants, Vol. I, No.* 70), which is at this instant in perfection, and the extract is made with cold water. The former, which by the above mentioned letter I learn you were about to receive, was made with hot water. The fruit to make this quantity of extract, four pounds and a half, cost sixpence, and the expense of making may be as much; this information will the better enable the Society to ascertain whether or not it can be useful to tanners or others in England. The rate of freight you can better determine than I can here.

Extract of the gaub fruit.

The little box is not quite full with the extract. I have filled it with calooee hemp, the produce of the second cutting of the same plants in two months, so I may safely conclude four crops or cuttings may be had annually.

Calooee hemp.

I am, &c.

W. ROXBURGH.

Calcutta, Nov. 3, 1807.

My dear Sir,

To convince you, that I have not forgot the Society nor you, I send you, above, copies of two letters which I have written to you since my return to this place. I also enclose a letter from the Secretary of the Asiatic Society, to convince you that I have been a faithful agent for establishing connections between the two Societies.

If you value our labours as we do ourselves, the original Calcutta price of each volume being fifty rupees, or half crowns, you will also see we have not been insensible of the attentions of the Society of Arts.

Sun hemp.

I am not relaxing in my pursuit after substitutes for hemp and flax; some more experiments are beginning, and are very far advanced, which promise success, that is, cultivating our sun or Indian hemp, during the dry season, as practised at or near Bombay, and at Malabar, where their sun or hemp has been reckoned in London equal to, if not better, than the best Russia hemp.

East India canvas.

Canvas is now made here in very large quantities by two or three clever Europeans, from the common sun plant of this country, of so very good quality, as to have nearly superseded English canvas throughout India. This is gaining a great point, if England should ever be pushed for the raw material, as the freight of canvas from hence to Europe will be trifling when compared to the freight of the raw article.

Lignum vitæ.

I was told in England that lignum vitæ was becoming scarce and dear. I am inquiring after a substitute, but hitherto without any pointed success. Should I meet with any kind of wood, that promises well, I will send the Society a specimen, and another specimen of a kind of very beautiful elegantly veined black wood for furniture, called here *seet-saul*.

Elegant black veined wood.

I remain, yours, very sincerely,

W. ROXBURGH.

Botanic Garden, near Calcutta,
Feb 9, 1808.

My dear Sir,

Resin of valeria indica.

Since my last, of the 9th of February, this year, I have got some farther matters to communicate. In the first place, this will be accompanied by a sample of the resin of the large Malabar tree, called by botanists *valeria indica*[*]. It appears to me to resemble amber more than copal. It may perhaps be very pure copal, and in this state, like amber

Electric by friction.

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[*] No. 1564 of my drawings of Indian Plants, sent to the Court of Directors.

when rubbed on paper, as I have this moment tried it, exhibits electric powers, by attracting small bits of paper, feathers, &c. I however do not mean to point out its qualities, but rather send this sample for information, requesting you to get it examined by some well-qualified persons, and let me know the result. Large quantities may be had in this country to send to Europe, if it is found useful, and will answer in price.

A similar resin sold for amber.

There is brought annually from Muscat in Arabia to this market considerable quantities of a similar resin, under the Persian name *kahroba*, which signifies *amber;* some of this I have also the pleasure to send you, and also beg to be informed of its nature and qualities. The purest pieces are susceptible of a fine polish, and are here cut into beads and ornaments, which are much worn by the natives as well as European ladies. I once saw a very beautiful string of these beads sent to England under the name of amber beads. The most beautiful amber-coloured pieces are therefore the most valuable, and are sold for about a shilling the pound by retail in the bazar. The less pure pieces and the green-coloured are at a much lower rate. My correspondent, who resides where the tree grows in the elevated lands of Malabar, sent me chiefly green pieces, thinking, no doubt, they were the most beautiful, and would therefore be the most acceptable.

Olibanum.

In the 9th volume of the Researches of the Asiatic Society at Calcutta is a paper on olibanum, by Mr. Colebrook, the president; some of this article, which he was so good as to give me to send to the Society, will accompany this letter; we both wish you would get it examined, and favour us with an account thereof as early as possible, particularly if sending it to London for sale can answer any good purpose. I have not yet got any thing which I think will answer for lignum vitæ, nor have I yet got the log of black wood (seet-saul, Hind.) mentioned in my last letter. In the same parcel with your three specimens above-mentioned, I have put one of the resin of *valeria indica*, and one of olibanum, for the Company's Museum, which I request you to send with my compliments to Mr. Wilkins, when you can furnish him with an account of their properties.

I remain, my dear Sir,

Yours very truly,

W. ROXBURGH.

Calcutta, April 8, 1808.

* * *

My dear Sir,

Fever bark.

Mr. Amos informs me, that more of my fever bark is wanted, I mean the swietenia febrifuga, (see Coromandel Plants, Vol. I, page 18, tab. 17,) of the properties of which I gave you particulars in March, 1806. I am sorry it is not in my power to send any from hence at present, as I have none by me, and the tree grows among mountains many hundred miles from hence. I left some when in England with Mr. Salisbury, at the Botanic garden, Brompton.

Caducay galls.

I wish to know the real value in England of the caducay galls, one of the most useful dyeing drugs known in this country, and of which a particular account is given in a letter of mine, inserted in the 23d volume of the Society's Transactions. If the Mediterranean trade should be obstructed, this article would be of great service in dyeing the manufactures of Great Britain, and particularly in the Turkey red dye upon cotton, as a valuable substitute for the Aleppo galls.

Orange dye.

I have at last got the orange dyeing drug, called wassuntagunda, for the Society, and Dr. Bancroft's experiments; it is a powder found on the seed-vessels of my *rottleria tinctoria*. See Coromandel Plants, Vol. II, No. 168.

Hard black wood.

I have procured a log of the hard black wood *(seet-saul)*, and have shipped it in the Georgina packet, Captain Leigh, to be delivered to Mr. Wilkins, the Hon. Company's Museum-keeper. It is reckoned the largest and most durable wood of this country; but still I fear it will be too soft to be a good substitute for lignum vitæ.

The wassuntegunda I have also sent under cover to Mr. Wilkins; he will no doubt send it to you.

You will receive from me soon a corrected copy of all my former experiments on indigo, with explanatory drawings. On my departure from England, I left with you some papers on various subjects, they may contain some matters deserving notice, when you have leisure to arrange them, as I left them in a rough, unconnected state, having not had time to put them in order.

Seeds preserved in gum arabic.

I mentioned to you in England, that I had frequently sent the seeds of vegetables from the East Indies to London, enveloped in thick mucilage of gum arabic, which was then suffered to dry with the seeds incorporated therewith; in this mode the vegetative power of the seeds is well preserved, it being necessary only when they are to be sown, that the mixture of gum and seeds should be put into water, which will redissolve the mucilage, and leave the seeds in a state ready to be put into the earth.

In consequence of the difficulty which subsists in the carriage of plants from England to the East Indies, I have enclosed some directions for preventing the accidents, which have hitherto occasioned great losses in their conveyance.

I am, my dear Sir,

Yours very sincerely,

W. ROXBURGH.

Directions for taking care of growing Plants at Sea.

Directions for taking care of growing plants at sea.

Particular care, if not placed in a cabin, must be taken, that they are kept covered during stormy weather, or such as raises the least saline spray into the air; for the chief danger plants are liable to at sea is occasioned by the saline particles, with which the air is then charged: these, falling on the plants, quickly evaporate, but leave the deadly salt behind; every care must therefore be taken, to guard against salt water and the spray at sea. During moderate weather, it will be proper to keep the boxes open, for plants cannot long exist without air and light, also during moderate rain, which is much better for plants than water from the cask, but too much moisture is more dangerous than drought. When the weather is dry, it will be necessary to give them a little fresh water now and then, the periods and quantity cannot be pointed out in any instructions, as the state of the weather must be the guide.

Directions where to place the chests cannot well be given, as that will in a great measure depend on the size, structure, &c. of the ship. In our Indiamen, round the capstan on the quarter-deck seems the best on many accounts. The greatest danger in such a situation is while the deck is washing in the mornings, the boxes must then be shut, and covered with a piece of canvas, or something to prevent the salt water getting in between crevices.

When plants from a cold climate get into a warm one, they shoot most luxuriantly, and often kill or choke one another; the longer shoots must therefore be frequently shortened, and as many of the leaves thinned as will give the rest air and room. Insects, particularly caterpillars, often make their appearance about the same time, they must be carefully picked off.

Roots and succulent plants.

Baskets with roots, (such as potatoes, &c.) or succulent plants, may be hung up in any cool, airy place, such, for example, as the projecting part of the deck which covers the wheel in our Indiamen, or hung over the stern, but in that case they must be covered with a tarpaulin or painted canvas.

Seeds.

Seeds ought to be kept in a cool, dry place, and never put below in the gun-room, hold, or lower deck.

Roots.

Roots ought to be packed in dry sand, after being moderately dried, and despatched in any ship that sails about the close of the year.

IX.

Cultivation of Poppies with Carrots[*].

Carrots advantageously

IN some parts of Germany poppy and carrot seeds are sown together. On light soils the poppy branches out but little, and its roots are scarcely sheltered from the strong heats. The carrot covers these roots with its leaves, and preserves them from drought, by retaining the moisture in the ground: at the same time it allows the poppy to enjoy the sun and air freely; and cannot injure it in the ground, as its root strikes perpendicularly downward, while that of the poppy ramifies near the surface. In this way the produce of the ground is doubled. An experiment shows, that the poppy is not injured either in the quality or quantity of its produce by this practice.

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[*] Sonnini's Biblioth. Physico-econom. Oct. 1808, p. 221.

cultivated with poppies.

Carrot seed was sown in the intervals between the poppies on a quarter of an acre of land. The harvest produced 3 simmers [near 7 bushels] of poppy seed, from which were expressed 12 quarts of clear and well flavoured oil, and 21 pints of thicker oil. The former, at the current price of 36 kreutz. [1*s.* 5*d.*] a quart, and the oil cakes, at 3 kr. [near 1½*d.*] apiece, fetched 40 fl. 42 kr. [£4 15*s.*]. This is exclusive of the thick oil, the carrots, and the tops of the carrots eaten by the cattle as fodder.

Calculation of produce.

X.

Method of preserving and keeping in Vigour Fruit Trees planted in Orchards or Fields.*

IT has been observed, that the numerous roots of the herbage growing round fruit trees, recently planted in fields and orchards, are injurious to the vegetation of these young trees; and their fruit is smaller and inferior in quality, in proportion to the quantity of the herbage that covers their roots. This is particularly the case with peach trees. In Germany, to prevent this, they surround the fresh transplanted trees with the refuse stalks of flax, after the fibrous part has been taken off, spreading it over the ground as far as their roots extend; and this gives them surprising vigour. No weeds will grow under this flax, and the earth remains fresh and loose.

Herbage injurious to the roots of young fruit trees

prevented by covering them with flax stalks.

This experiment has been tried on an old peach tree, lan-

This revives

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* Sonnini's Biblioth. Physico-économ. Sept. 1808, p. 161.

the vigour of old trees.

guishing in an orchard. Refuse flax stalks were spread at its foot, and far enough round to cover all its roots; when it soon recovered its strength, pushed out vigorous shoots, and was loaded with larger and better fruit than before.

Dead leaves answer the same purpose.

The leaves of trees falling in autumn may be employed in the same way with advantage; but dry branches, or something else, should be laid over them, to prevent their being blown away by the wind. The leaves of walnut trees appear to produce the best effect.

SCIENTIFIC NEWS.

Recent discoveries.

THE substance of the late discoveries communicated by Professor Davy to the Royal Society is as follows.

Oximuriatic acid

1. That the oximuriatic acid is a simple body, belonging to a class, in which two bodies only at present are known, this and oxigen.

analogous to oxigen.

2. That like oxigen it forms bodies, which are either acids, or analogous to acids, or oxides, by combining with combustible bodies.

Muriatic acid.

3. That hidrogen is the basis of the muriatic acid, and that oximuriatic acid is its acidifying principle.

New compounds.

4. That phosphorus, sulphur, tin, arsenic, &c., by combining with oximuriatic acid, form substances analogous to acids, which have the power of neutralizing ammonia, and probably other alkalis, and of forming combinations with other compounds of the same class.

One analogous to an earth.

5. That phosphorus acidified by oximuriatic acid forms a compound with ammonia not decomposable by a white heat, and having characters analogous to an earth.

Compounds of oximuriatic acid.

The combinations of oximuriatic acid with inflammable bodies offer objects of investigation of a perfectly novel kind, analogous to, and scarcely less interesting than those belonging to the combinations of oxigen.

More than one acidifying principle.

The chemists of the phlogistic school supposed only one principle of inflammability. Lavoisier, in his beautiful generalization, was acquainted with only one acidifying principle, or one principle which rendered bodies soluble: but there is actually another known, viz. oximuriatic acid; and it is not impossible, but others may be discovered.

London Hospital.

Dr. Buxton's lectures on the practice of medicine, will be commenced on Monday, the 1st of October. Medical lectures.

St. Thomas's and Guy's Hospitals.

The autumnal courses of lectures at these adjoining hospitals, will begin the first week in October: viz.

At St. Thomas's.

Anatomy and the operations of surgery, by Mr. Cline and Mr. Cooper.—The principles and practice of surgery, by Mr. Cooper.

At Guy's.

Practice of medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental philosophy, by Mr. Allen.—Theory of medicine, and materia medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and diseases of women and children, by Dr. Haighton.—Physiology, or laws of the animal economy, by Dr. Haighton.—Structure and diseases of the teeth, by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *complete course of medical and chirurgical instructions.*

To Correspondents.

My correspondent R does not seem to be aware, that the heights of the barometer are not taken at the same time of the day by Mr. Gilpin and Mr. Bancks.

It does not appear when Zahn's experiment on the radiation of cold was made, from the account of Prof. Kries, who only mentions it incidentally; and I have not his works to refer to. I do not think it necessary therefore, to copy from Musschenbroeck the account of a similar experiment made by the Academy del Cimento, and published in their transactions for 1667.

Mr. Knight's paper is necessarily deferred on account of the plate.

METEOROLOGICAL JOURNAL,

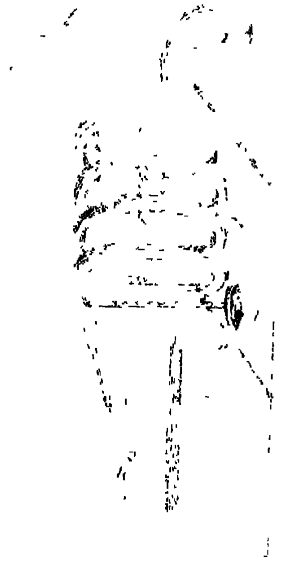
For AUGUST, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

JULY Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night	9 A. M.	Day.	Night.
27	60°	57°	62°	52°	29·63	Rain	Cloudy
28	60	55·5	61	51	[illegible]	Ditto	Fair *
29	58	59	63	56	[illegible]	Ditto	Cloudy
30	62·5	60	64	52·5	29·71	Ditto	Ditto
31	60	58·5	65	49	29·83	Ditto	Fair
AUG.							
1	57	59·5	62·5	52	29·82	Ditto †	Ditto
2	59	62	66	57·5	29·98	Fair	Cloudy
3	62	63	69	55	29·87	Rain	Fair ‡
4	61	60·5	68	53·5	29·62	Ditto	Ditto
5	59	57	63·5	49·5	29·64	Ditto §	Ditto
6	57·5	61	66	56·5	29·77	Fair	Rain
7	62	61·5	65	57	29·65	Rain	Cloudy
8	61	60	65	52	29·57	Ditto	Ditto
9	58	59·5	62	57	30·00	Fair	Ditto
10	62·5	62	69·5	52	29·79	Ditto	Ditto
11	60	59	66·5	53	29·64	Rain	Ditto
12	60	62	67	55	29·87	Ditto	Cloudy
13	59	61	65	56	29·54	Fair ‖	Ditto
14	62	61	64	54·5	29·83	Cloudy	Ditto
15	57	57	63·5	46·5	29·55	Rain	Ditto
16	52	52	54	47	29·52	Ditto	Ditto
17	53·5	55	60·5	45·5	29·87	Fair	Fair
18	55	59·5	66·5	53·5	30·20	Ditto	Ditto
19	59	58	63	49	30·18	Rain	Fair
20	58	64·5	69	51	30·28	Fair	Ditto
21	59	64	70·5	50·5	30·24	Ditto	Ditto
22	59	64·5	72	51	30·16	Ditto	Ditto
23	62·5	67·5	73	58	30·02	Ditto	Ditto
24	64·5	68·5	75·5	59	30·06	Ditto	Ditto
25	64	68·5	73	59	30·02	Ditto	Ditto ¶
26	62·5	67	70	57	29·97	Cloudy **	Ditto

* Rain, A.M. and P.M. Cold evening.
† Thunder at 5 P.M.
‡ Rain at 11 P.M.
§ Tremendous thunder, vivid lightning, and heavy rain, about 1 P.M.
‖ Boisterous morning.
¶ Cloudy at 11 P.M. with cool breeze.
** The morning only.

[illegible] Cutting Instrument



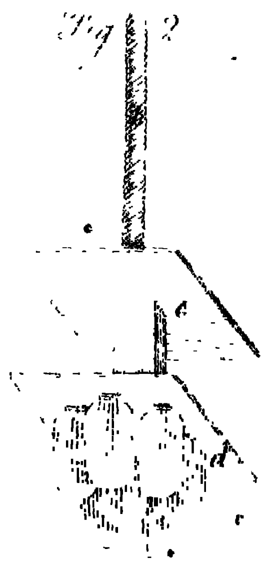


Fig 2

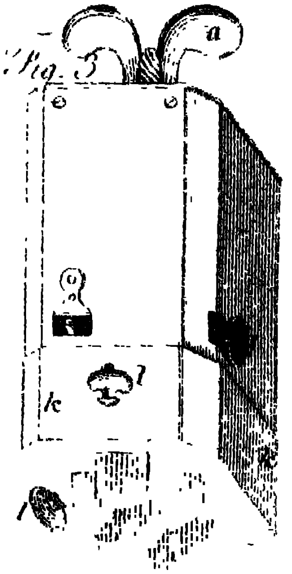


Fig 3

Mr. Knight on Capillary Attraction

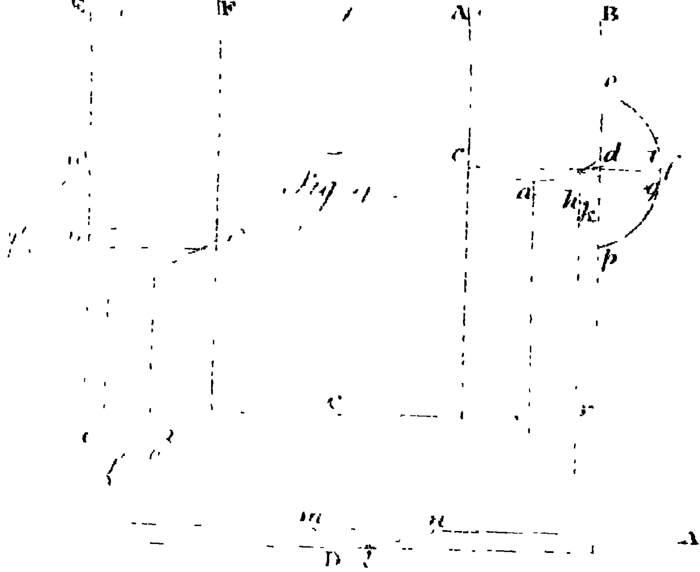


Fig 4

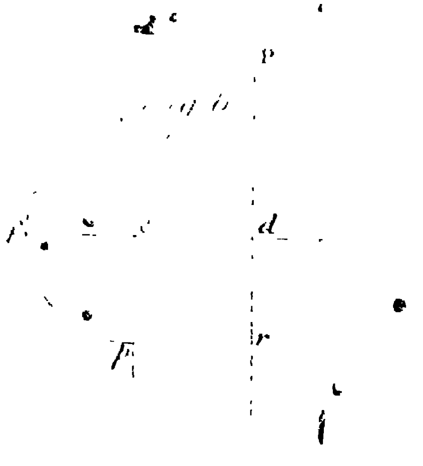


Fig 6

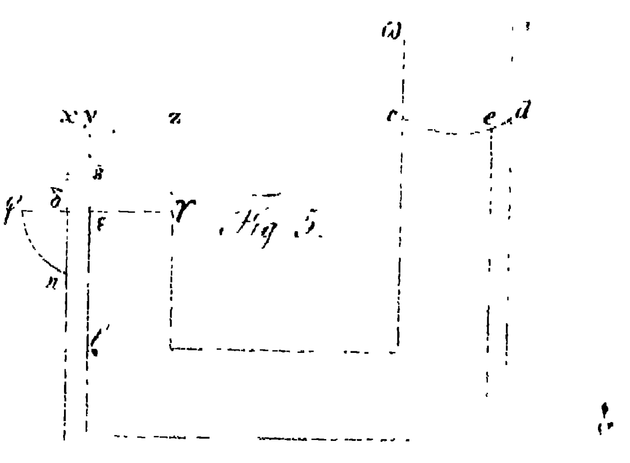
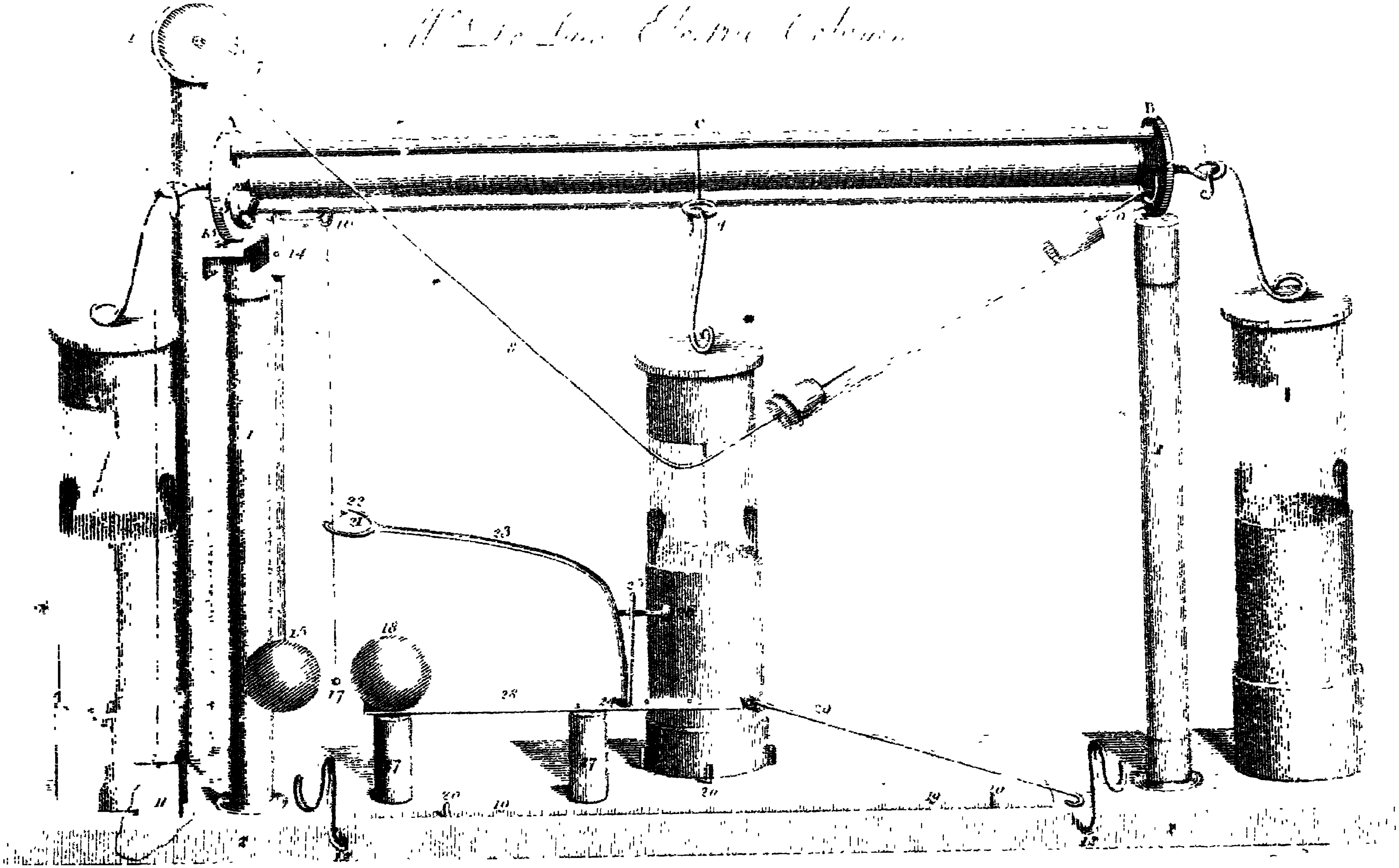


Fig 5

Mr. De Luc's Electric Column



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1810.

ARTICLE I.

On the Electric Column and Aerial Electroscope. By J. A. De Luc, *Esq. F.R.S.*

Galvanism shown to be a modification of electricity by Volta's pile.

THE principal result of my paper *on the Analysis of the Galvanic Pile* has been to show, that by this instrument, in which Sig. Volta has so much extended Sig. Galvani's first discovery of some physiological effects produced by two associated metals, we have been really enabled to determine whence proceeded that action upon the animal economy. When these effects were discovered by Sig. Galvani, appearing similar to the *shock* produced by the Leyden vial, they pointed out some action of the *electric fluid*: but when this *fluid* acts thus upon our organs, it is also manifested by *electric motions* and by *sparks*; whereas not even the first of these signs appeared in the galvanic experiments. Therefore the action of the *electric fluid* in these first phenomena might for ever have remained doubtful, had not Sig. Volta, by the invention of his admirable *pile*, increased that action, so as not only to be attended with *electric motions* and *sparks*, but to produce some *chemical effects* known before to belong to the *electric fluid*.

Two distinct effects shown in the pile.

But there remained a great question. These different effects had before been produced by the *electric fluid* only when it was arrived at a great *density*; while the *pile* produces the same effects with so small a quantity of the *fluid*, as to be often hardly sufficient to move the *gold leaf* electroscope. This has been the object of the experiments contained in my first *paper*, which have manifested two distinct effects in the *pile*: 1. A *motion* of the *electric fluid* produced by the association of two proper *metals*, independent of any other effect: 2. A *modification* of this small quantity of *electric fluid*, on pervading the *pile* during the *calcination* of some of its *metals*. A distinction first shown by different *dissections* of the *pile*, and afterward directly proved, by producing an instrument, which retains the *electric effects* of the *pile* by the association of two *metals*, without either its *chemical* effects, or the *shock*.

A new instrument,

the electric column,

manifesting some variable influence of the electric state of the air.

This new instrument was to be distinguished from the galvanic (or voltaic) pile; therefore, in my second *paper* delivered to the Royal Society on this subject, I named it *electric column*, as being a spontaneous and permanent *electric machine*; and as it manifested also, by changes in its *electroscopes*, some variable influence of the *electric state* of the *air*, this new effect was to be expressed by an additional name. It is different from the indications of our former *atmospheric electroscopes*, such as elevated *conductors* and *kites*, which inform us only of the *comparative* states of the *stratum* of *air* that they attain, and of the *air* at the place of observation; without any indication of the *changes* in the latter, probably connected with some pheuomena, which we observe; without knowing their cause; and as the *electric column* seemed to promise a method of discovering these *changes*, I named it also *aerial electroscope*.

Object of the present paper

Such had been the principal object of my second *paper* presented to the Royal Society; it was only in its nascent state; but as I thought it worthy to be taken up and followed by other experimental philosophers, I would not postpone communicating it to the public till it was more advanced: it has not been published in the Phil. Trans., and as I have since carried it farther, I shall here treat it in a different manner, dividing it into three parts: the *first* will relate to the *electric column*, considered only as the *electric efficiency* in the *pile*, divested of either *chemical* or *physiological* effects. The *second* will explain the difficulties which I have encountered in attempting to bring the instrument to its desirable function as *aerial electroscope*, and the point which I have attained in this new kind of *meteorological* observations. And in the *third*, I shall offer to the attention of the natural philosopher, some *meteorological* observations, which show the importance, in every branch of experimental philosophy, and especially in chemical theories, of forwarding the observation of atmospheric phenomena.

PART I.

On the Electric Column.

Cause of the effects of the pile.

I have explained in the first *paper* my system concerning the cause of a *motion* of the *electric fluid*, produced by the properties of two associated *metals*; and as all the circumstances attending this *motion* are characteristic of its cause, this connexion will be here my principal object.

I shall begin by some experiments under the same form as Exp. 4 in the former paper, in which the *brass tripods* were placed between the two *metals*, and these *groups* separated by pieces of *wet cloth*. In this first dissection of the *pile*, both *electric* and *chemical* effects were produced, the latter on account of the *water* contained in the cloth; and I wanted to know what would be the consequence, in the same arrangement, with respect to the *electric* effects of suppressing the *chemical* ones, by a *dry* intermediary substance. For this purpose, I substituted for the *silver* plates and the *wet cloth* of the former experiment pieces of *Dutch gilt paper*, placing the *brass tripods* between the *zinc* plates and the *copper* side of the papers; and after having found, that 40 such groups could be contained in each column of the frame described in that paper, forming in the whole 80 groups, I made the following experiments.

Electric and chemical effects.

The chemical suppressed.

Exp. 1.

Exp. 1. In order to judge what would be the effect of interposing the *brass tripods* between the *metals*, I first mounted the 80 groups without them, and observed the degree of divergence of the *gold leaves* at both extremities. *Dry* paper not being so good a *conductor* as *wet* cloth, the *electric* effects were not so great as they would have been with the latter; but they were sufficient for my purpose. I kept this apparatus for some days, observing the divergences in different parts of the day, and the greatest, which happened to be at the *zinc* side, was of 0·3 of an inch.

Exp. 2.

Exp. 2. I then placed the *brass tripods* between each *zinc* plate and the *copper* side of the papers, the *paper* side of which separated the *binary* groups of *metals*, as did the *wet cloth* in the former experiment: the *electric effects* remained the same as they were in *Exp.* 1, without the *tripods*.

Effect of the air.

It came then into my mind, that the *tripods*, being of *brass*, might alone produce some effect, with only plain *paper* to separate these groups of *zinc* and *brass*; but the latter having but a small mass, the transmission of *electric fluid* along the *column* could not but be *slower*; and this was the very reason which determined me to the trial, as an experiment that would also relate to the cause which renders the *column* an *aerial electroscope*: this cause is the action of the *ambient air*, the immediate effects of which are, to lessen the *positive* state of one of the extremities, and the *negative* state of the other, according to its own *electric* state; and more *slowness* in the *motion* of the fluid giving more *time* to this action of the *ambient air* for diminishing the *electric* indications at the extremities of the *column*, these symptoms were to be smaller. This therefore induced me to make the following experiment.

Exp. 3.

Exp. 3. I mounted the column with 80 groups composed of *zinc* plates with only the *brass tripods*, separating them with pieces of *writing paper*, and I kept also this column for some days, observing the *electroscopes* at its extremities: they had the same variations which I had before observed, but very small, and the greatest divergence, which, as it is commonly, was in the middle of the day, did not exceed 0·1 of an inch.

Slowness of the motion of

I now come to that *slowness* mentioned above in the *motion* of the *electric fluid* produced by the property of the *column*, the fluid in the column, from its tendency to adhere to bodies.

column, which being attributed to a *fluid* known to possess, in the proper sense, the swiftness of *lightning*, must appear a paradox. Rapidity of motion certainly belongs to the *electric fluid*, when darting in a torrent; but the *electric matter*, of which it is composed, has a tendency to adhere to all bodies, *air* included; and it is this very property, as explained by Sig. Volta, which occasions the *electric motions*, when the *fluid*, tending to move by a rupture of its *equilibrium*, is more reluctant to be separated from the body which possesses it, than the latter to follow the *fluid* in its motion.

Analogy in water.

The effect of this tendency of the *electric fluid* towards bodies, in retarding its *motion* within the *column*, is analogous to an effect observed in water. When *water* is kept in motion in a channel by a constant supply, it is seen to take its course in the middle, leaving behind the particles retarded by their tendency towards the sides, which is decreasing as the distance increases. But the analogy is more direct, when *water* is confined in a pond, beset with aquatic plants or other impediments; for the *motions* impressed on that *water* at one side of the pond, though continued, if small, are but *slowly* and seldom *completely* communicated through the whole space. The case is the same with respect to the *electric fluid* set in *motion* by the property of the *column*, not only when confined within it, but when a *current* is produced; which effect will be shown by some experiments, after I shall have explained some parts of the figure annexed to this paper. See Plate III.

Explanation of the plate.

The dimensions of this *figure* are half those of the original: it consists of many parts which I shall successively describe, beginning by those which relate to my present object: its fundamental parts are an *electric column*, with its *electroscopes*. The former is represented at A, B, supported horizontally on two pillars 1, 1, consisting of solid glass rods, covered with *sealing wax*, or with some other insulating varnish, and fastened on the wooden base 2, 2, by female screws underneath. The *column* itself is composed of 600 groups, formed of *zinc* plates 0·7 inch diameter, and equal pieces of plain *Dutch gilt paper*; the *copper* side of which being turned towards A, this is the *positive* extremity of the *column:* and as also, in every group, the *paper* itself serves only to separate the *binary* groups of *zinc* and *copper*, the latter being in each of them on the side of B, this is the *negative* extremity. The groups are contained between three glass rods, covered with sealing wax melted over them while hot, and fixed in holes of the brass plates A, B, where they have been introduced while the plates were hot, and the holes filled with sealing wax. These brass plates have in their lower part a pin, which enters freely into the brass cap at the top of the pillars 1, 1. At the extremities of the *column* are screws 3, 3, formed on the outside in the shape of loops: they serve first, to press the groups between the glass rods; and besides to produce, by brass wires hooked in their loops, the communication of each extremity with the nearest *electroscope,* as represented in the *figure.*

This column too powerful for some experiments.

In general this *column* produces too great effects for the experiments which I have here in view, as the *gold leaves* strike the sides of the electroscopes; while there should be merely a simple *divergence*: therefore, either a smaller *column* must be used, or the time must be chosen when the 600 groups produce only this effect.

Exp. 4.

Exp. 4. Having observed the actual quantity of *divergence* in both electroscopes, when I lay my finger on the top of either of them, in order to produce the communication of its extremity of the *column* with the ground, the *divergence* ceases in it, and becomes nearly double in the other. Then taking off my finger, and thus abandoning the *column* to its own operation, the divergences are not immediately restored to their former quantity; it requires some time to produce them again, even half an hour or an hour.

Slow motion of the fluid does not take place in all cases.

This shows a *reluctance* in the parts once possessed of the *electric fluid,* to obey the cause which requires more of it on *zinc* than on *copper,* in order to establish the *electric equilibrium* between them. But this concerns only the quantity of *electric fluid,* which the *column* possesses in common with the surrounding bodies and the ambient air; for at the same time that this quantity delays to obey the law of the *column,* if an insulated body, either *positive* or *negative,*

negative, but in a degree which can merely affect the gold leaf electroscope, be applied to one extremity, the effect is instantly perceived at the other. This again is the same as if a stream of water were introduced at one side of the *pond* of the above example, and an opening made at the opposite side; for a *current* would be directly produced through the *pond*.

Exp. 5.

Exp. 5. After having disturbed the state of the *divergences* at the extremities of the *column*, by placing a finger on one of them, the mode of reproducing speedily the former state is, to lay the fingers on both extremities together, and remove them *at the same instant*: if this last condition be really obtained (which is not easy) the original *divergences* are restored.

A fluid set in motion.

These experiments cannot leave any doubt, that the phenomena of the *column*, as well as the *electric* part of those of the *pile*, are produced by a *fluid* set in *motion*.

Exp. 6.

Effect like those on the leaves of the sensitive plant.

Exp. 6. In the same case as that of the above experiments, namely, when the *divergences* are not too great, if one side of the *column* be placed in communication with the ground, the effects of the contact on the other side are so similar to those produced in the same manner on the *leaves* of the *mimosa sensitiva*, that this conformity of effects seems to indicate some analogy between the causes: both contacts make the *leaves* fall; they rise again, but it requires some time. There is an objection against the idea that the phenomenon of the plant is *electric*; because in the *column* the same effect is produced, at one side by *imparting*, and at the other by *taking off*, some *electric fluid*. This objection however is not absolute, for we do not know all the actions of *organic bodies* on the *electric fluid*; but if it is not this, it must be some other *fluid*, which is acted upon by the contact of the plant. In general, we are very little advanced in the knowledge of the subtile *agents* operating in terrestrial phenomena; and as we cannot make any real progress in this knowledge but by endeavouring to increase, by observation and experiments, the number of the phenomena which have analogies with each other in some respect, it might be useful to follow an attentive comparison, at different times and in different arrangements of circumstances, between the effects of contact on the *gold leaves* of the *column*, and on the *leaves* of the *mimosa sensitiva*, and even contact with different bodies.

Stronger proofs of a moving fluid.

I come to greater symptoms of the *motion* of the *electric fluid* in the *column*, beginning by experiments, which will prove what, in the former *paper*, I have concluded, from my theory, namely, that by the cause assigned to this *motion*, the *negative* effect goes on increasing from the *zinc* to the *copper* extremity of the *column*, at the same time that the *positive* effect increases from the latter to the former; and that the *electric state* of each point of the insulated *column* is the *sum* of the correspondent terms of two inverse series of progress represented by determined, though in some respects variable, numbers, in a *table* given in that *paper*.

Addition to the apparatus.

For these experiments a third *electroscope* is used: in the *figure* it is seen connected with the middle point C of the *column*, where is a thick brass plate with a projecting loop, 4. This immediate connexion of the electroscope with the middle point of the *column* serves for some experiments; but every other point of the *column* may be made to communicate with it, by the interposition of a soft wire held in the middle by an insulating handle. When this is used, the communication of the electroscope with the middle point is taken off; and, by bending the wire, it is easily made to connect, as *conductor*, the necessary parts of the *column* with this *electroscope*.

Exp. 7.

Exp. 7. At a time when there are simple *divergences* in the electroscopes at the extremities of the *column*, if they be equal, *positive* at A, and *negative* at B, there is no *divergence* in the electroscope at C; this is *neutral*; which is the case expressed in *Table I* of the former paper: and if at this time any point of the *column*, at a distance from the point C, on the *negative* or *positive* side, proportional to one of the terms of the *table*, be tried with the insulated *conductor*, the *divergence* which it produces will be found, as exactly as can be expected in such experiments, correspondent to the *number* expressed in the *table*, with its *sign*.

Exp. 8.

Exp. 8. In this situation of the *column*, the states expressed in *Table II* and *Table III* may be also observed by placing alternately its extremities in communication with the *ground*; but by a *wire*, because the metallic *chains*, commonly used for this purpose, do not transmit completely such small quantities of *electric fluid*, probably on account of some dust getting between the links. The following are the two cases of this experiment.

A wire conducts better than a chain.

1. When the communication with the ground is made at B, the electroscope at the middle point C diverges *positively*, in the same degree as did before the electroscope at A; and the divergence of the latter is now nearly *double*. I say nearly, because equal increases in the *electric state* produce smaller increases in the *angular* motion of the *gold leaves* in proportion as the *angle* increases. By using then the small insulated *conductor*, it is found, that the whole *column*, (except the very extremity B, which, communicating with the *ground*, is *neutral)* is in a *positive* state, increasing towards A: which is expressed in *Table II.*

Divergence of the electroscope not in the precise ratio of the intensity.

2. When the communication with the *ground* is made at A, the *electric states* of the *column* are all reversed. The electroscope at the middle point C has now a *negative* divergence, equal to that of the electroscope at B in the insulated state of the column; and the divergence at B is nearly double. Then, by observing the state of the other parts of the column with the insulated *conductor*, the *negative* state is found increasing toward B, from A, the only point not *negative*, but *neutral*. This is the case represented in *Table III.*'

In the three different cases above described, the indicated *positive* and *negative* states are, in every part of the *column*, common to *zinc* and *copper*. There is no doubt, in every binary association of the *metals*, that *difference* between them which their nature requires; but it is insensible in them individually, as it is when they are single; and their *electric state*, embracing both *metals*, is determined, according to their position in the *column*, by the *motions* of the *electric fluid* resulting from these insensible *elements*; and that they follow the laws determined in my paper from the cause assigned, is verified by these experiments; which demonstrate at the same time, that there are no *positive* or *negative* states belonging to any part of the *column* (nor consequently

Both states of electricity common to both metals.

consequently of the *pile*); since each part may change from *positive* to *negative*, or inversely, according to circumstances, by the different *motions* impressed on the *electric fluid*; which *motions* may be concluded from these phenomena themselves, but will be directly perceived in the following experiment.

Exp. 9.

Exp. 9. A necessary condition of this class of experiments is, that the state of the *ambient air* be such, that alternately, at each extremity of the *column*, one of the *gold leaves* strikes the side of the electroscope, and at last *sticks* at one of the extremities. The following are three different cases in these phenomena.

1. When the *strikings* are alternate at the extremities, these instantaneous communications of the *column* with the *ground* at each side, by the contact of the *gold leaf* with the *tin foil*, produce in the former a *flux* and *reflux* of *electric fluid*. When the *gold leaf strikes* at the *copper* extremity, some *fluid* ascends from the *ground* into the *column*, and repairs the deficiency on this side; but this additional quantity of *electric fluid* in the *column* occasioning a *striking* at the *zinc* extremity, the new quantity of *fluid* returns that way to the *ground*. These *rises* and *ebbs* of the *electric fluid* in the *column* are observed at the *middle electroscope*, but only when some time elapses between the *strikings*; for, on account of the *slowness* of *motion* of the *fluid*, directly shown above, when the *strikings* rapidly alternate, before one of the effects has extended itself in the *column*, the contrary effect begins; in the same manner as the *rise* and *ebb* of the water are not sensible within the *Mediterranean sea* and the *Baltic*, on account of their narrow entrance; for before the *flux* has extended itself some way up these seas, the *reflux* operates in the contrary direction. But these *motions* of the *electric fluid* are very sensible at the *middle point*, in the following cases.

Cause of the want of tides in the Mediterranean and Baltic.

2. When the *gold leaf* comes to adhere at the *copper* extremity, thus placing it in a continued communication with the *ground*, the *strikings*, which become more frequent at the *zinc* side, produce a *pulselike stream* of the *electric fluid* in the *column*, manifested by the motions of the *gold leaves* in the *middle electroscope*: their divergence is *positive*,

the whole *column* being now in this state (*Exp.* 8, 1.), but they *fall* in part, when the *gold leaf* strikes at the *zinc* side, and *rise* in the intervals of the strikings; thus pointing out clearly a *current* flowing from B to A, at a *higher* level than the *standard*, which level alternately *rises* and *falls*.

3. When the adhesion of the *gold leaf* takes place at A, the *zinc* side; which circumstance, producing a continued communication of this side with the *ground*, renders the column *negative* in the whole (Exp. 8, 2.); while the *gold leaf* at B, the *copper* side, goes on *striking*; a *current* of *electric fluid* is also produced, but at a *lower* level than the *standard*: the divergence of the *gold leaves* in the *middle electroscope* is consequently *negative*, and, as in the former case, they *fall* in part at every *striking*, and *rise* in the intervals; but while in that case they *fell* by the *lowering* of level of the *current*, and *rose* when it came *higher*; now diverging as *negative*, they *fall* at every *striking*, because some *fluid*, ascending from the *ground*, makes the column less *negative*; and they *rise* again while this fluid flows into the *ground* by the *zinc* side, and thus prepares another *striking*.

Motions of the fluid in the pile more confused and less lasting.

I have made the same experiments on the *motions* of the *electric fluid* within the *pile* itself; they are more confused and less lasting on account of the calcination of the metals: but the *column*, being in fact the *electric machine* of the *pile*, shows clearly and permanently these *motions*, which I shall now follow in the *circuit*, or when the two extremities of the *column* (or the *pile*) are connected together by some intermediate body. In this case the *motion* of the *electric fluid* is manifested by more or less *retardation* of its *current*, according to the degree of *conducting* faculty of the body employed.

Additional apparatus.

For this class of experiments (see the *figure*) brass hooks, 5 and 6, are fixed to the small brass plates, terminating the *column* at each extremity, and against which press the screws: these hooks project a little more than an inch, and serve for different purposes. The following experiments will relate to the *conducting* faculty of that kind of *glass tube* filled with *water*, entered by *wires* on both sides, in which *chemical* effects are produced when it is applied to the *pile*; but with the *column*, these effects do not take place.

Exp. 10.

Exp. 10. The tube of the above kind, which serves in this experiment, is represented in the *figure*, as suspended at the point 7; its wire, 8, having a hook, held up by a silk thread, which, passing over the pulley, 10, descends to a thin brass plate, 11, fixed to the base of the instrument. This brass piece bends forward at the top, and the silk, entering into a notch of this projection, is stopped there by a bead fixed to it. The other wire, 9, of the tube, is hooked on the projecting wire, 6. In this situation of the *tube*, it does not affect the *electroscopes* of the *column*, they continue to diverge as if the *tube* were not connected with one of them; but when, the silk being disengaged, the hook of the wire, 8, comes to rest on the hook, 5, of the *column*, the *circulation* of the *electric fluid* produced through it between the extremities A and B is so rapid, that the *divergence* entirely ceases in the *electroscopes*; and it returns only, when the extremity of the wire, 8, is again lifted up. This shows, that the *glass tubes* of this kind are sensibly as good *conductors* as *metals*.

Permeability of bodies to electricity.

The different *conducting* faculties of bodies proceed from different degrees of *adhesion* of the *electric fluid* to them; but beside this difference among bodies, there is another, which relates to *permeability*. All the bodies, which I have tried, are *permeable* to the *whole* of the *electric fluid*, except those that can be *charged*; which are *impermeable* to the *electric matter*, and *permeable* only to the *vector*. This operation, called *charge*, as I have explained and proved in my works, consists in accumulating the *electric matter* only on one side of the *laminæ* made of these substances, by occasioning a proportional diminution of its quantity on the opposite surface, which is an operation of the *vector*; and the reason why other bodies cannot be *charged* is, that, the *electric matter* pervading them, though slowly in some of them, no sensible difference in the quantity of *electric matter* can be produced between their opposite surfaces.

Charged bodies.

Glass impermeable to the fluid, but it moves along its surface.

Glass in this respect, is a remarkable substance. It is absolutely *impermeable* to the *electric matter*, and, being a solid body, it is used for insulating pillars in our electric apparatusses; but the *electric matter* moves easily along its surface, as I have visibly shown by the *Lichtenberg figures* produced on its naked surface, where they dissipate in a little time, while they last many days on *resinous* surfaces. This is the reason of covering the *glass* pillars destined to insulate with some *resinous* varnish; but all these varnishes are not equally fit for the purpose, and this is one of the objects of the following process, as well as the trial of the different *conducting* faculties of other bodies.

Varnishes differ in properties.

For these experiments, the bodies to be tried must be reduced into slips or rods, which are to be laid on the hooks, 5 and 6, of the *column*, in order to observe the effect produced on its *electroscopes*; but there are necessary precautions to be used in laying them on the hooks. For instance, in respect to the bodies with which I shall begin, those which have different *conducting* faculties, belonging mostly to the vegetable and animal kinds, when they are placed on the hooks with the *fingers*, as it is almost impossible to lay them on both hooks at the same instant, the end which touches first disturbs the *equilibrium* of the *electric fluid* in the *column*; and I have shown above, that it is but slowly restored. In order to obviate this defect, and for another purpose that will follow, two brass wire brackets, 12, 12, are fixed in the front of the base of the instrument, on which the slips are first laid, and there taken up by two glass hooks covered with sealing wax, with which they are placed on the hooks of the *column*. I shall give a general idea of these trials under the following head.

Trials of the conducting powers of bodies.

Exp. 11. The substances of this class having more or less *conducting* faculty, they lessen in different degrees the *divergence* in the electroscopes, by transmitting more or less *electric fluid* from A to B. This is a curious kind of experiments, but as the particulars are not here my object, I shall relate only one, concerning the physiology of vegetables, which may lead to others of the same kind. Having repeated in presence of Dr. Lind these experiments on the different *conducting* faculties of various bodies, I showed him a phenomenon, which had surprised me. A thin slip of *deal*, cut *along* the *fibres*, being applied to the *column*, there remained but little *divergence* in the electroscopes; while a slip of the same wood, of the same thickness and breadth,

Exp. 11.

Difference in deal cut lengthwise & across.

breadth, but cut *across* the *fibres*, produced much less diminution in the *divergence*. Dr. Lind found probably the cause of this difference, assigning it to the situation of the *resinous* substance within that wood: it does not belong to the *fibres* themselves, since they transmit easily the *electric fluid*; it is lodged between the *fibres*, and thus forms an impediment to the passage of the *fluid* from *fibre* to *fibre* in the slips cut *across* them.

Trials on the insulating powers of bodies.

When these experiments are made with the view of trying the *insulating* property of bodies, still more precaution is required in placing them on the hooks of the *column*: for the bodies fit for this use being fundamentally *impermeable* to the *electric matter*, their *electric state* is changed, more or less permanently, by *friction*; and this in the manner which I intend to explain in a future paper. As, however, they can hardly be handled without some *friction*, they act upon the column by their *influence* (an effect that I shall show directly hereafter), and their *insulating* property cannot be observed, on account of the disturbance which they produce in the state of the *column* itself. These bodies therefore must remain a little time *untouched* on the brackets, and be there breathed upon, in order that the *moisture* of the breath may dissipate their *electrization*; serving as a *conductor* for their whole surface to the ground, through the brackets; and when the *moisture* is evaporated, the rods are taken up there with the insulating hooks, and thus applied to the *column*.

These experiments are particularly useful for a better knowledge of *insulation*, a point very important in the construction of electric apparatusses; for many experiments fail for want of a complete *insulation*; and I do not know of any shorter and surer method of trying the *insulating* faculty of *varnishes* laid on glass for this purpose, than that of applying to the *column* rods of *glass* covered with them. I shall also give only a general idea of this class of trials under the following head.

Exp. 12.

Exp. 12. It is very seldom, that a naked *glass* rod, being placed on the hooks of the *column*, does not sensibly diminish, in a little time, the *divergence* in the electroscopes, by transmitting slowly some *electric fluid* from A to B: but this is more or less, according to the nature of the glass; therefore these differences may become a particular object of experiments, concerning the property of different *glasses* for electric purposes, by comparing the effects of different rods on the electroscopes of the *column*; much care being taken, that all effect of *friction* be dissipated. With respect to *varnishes*, a proper *sealing wax* laid over the *glass* when *hot* is the best coating which I have found; a rod of this kind produces no change in the electroscopes. But all *sealing waxes* have not the same property, and before I had devised this mode of trial, I was sometimes disappointed in the construction of the small set of electric apparatusses, which I have mentioned in the former paper, even in that of the *electroscopes* of the *column*, as the top of their *glass* bottle must be *insulating*. The *sealing wax* reckoned the *finest*, because it melts more easily and spreads more smoothly, is not fit for this purpose, its softness being produced by spirit of wine. In general, for this essential choice of an *insulating* coating on *glass*, the column is very useful; for by laying the different *coatings* on *glass* rods, and placing these on the hooks of the *column*, those which will be found to diminish the *divergence* in the electroscopes are not completely *insulating*, and that *coating* must be used, by which the *divergences* are not affected.

Sealing wax the best varnish,

but not the finest.

Effects of friction on the insulating rods.

I come to the impression produced on the *column* itself, when there remains some effect of *friction* on the *insulating* rods applied to it. The experiments on this object will, at the same time, afford a new verification of the cause which I have assigned to the *motion* of the *electric fluid* in the *column* (or the *pile*), by what is called the *electric influence*; the *laws* of which, first really determined by Sig. Volta, I have explained by some modifications of the *vector*, which will be perceived in the following experiments; showing at the same time the effect of the *ambient air*, such as I have determined it. The proper time for these experiments is also when, from the *electric state* of the *ambient air*, there is not much *divergence* in the electroscopes of the *column*. They may be made with naked *glass*, which *friction* renders *positive*; and with a *glass* rod covered with *sealing wax*, which thus becomes *negative*. I shall explain the phenomena produced by the latter, because it retains longer the effect of *friction*; those produced by naked *glass* are of the same nature, only reversed.

Exp. 13.

Exp. 13. A glass rod covered with *sealing wax* must be rubbed so gently, that, when applied to the *column*, it only increases the *divergence* of the gold leaves at the *negative* side, without any *striking*, else the effects would be confused: when it produces the proper effect, the phenomena are the following.

1. At the moment when the *rod* is placed, the *divergence* increases at the *negative* side, and diminishes, or even ceases at the *positive* side. The cause of these effects is, that the *expansive power* of the *electric fluid* is lessened in the whole *column*, by part of its *vector* passing to the *negative* rod, where its quantity is *less*: and as however the *equilibrium* of the *electric fluid* in the *column* requires more *electric matter* on *zinc* than on *copper*, the latter, at the first moment, loses more of it, but not sufficiently to compensate the loss of *expansive power* at the *zinc* side; therefore less *electric fluid* passes into the *gold leaves* of the latter, which fall in consequence of this diminution.

2. Within a little time, the *positive* divergence is renewed at the *zinc* side, and the *negative* lessened at the *copper* side. This effect is produced by the *ambient air*, which, during the diminution of *expansive power* in the *column*, yields to it some *electric fluid*, especially to the *copper* side which was become strongly *negative*; and thus the former *equilibrium* is restored in the *column*.

3. The proof of the above explanations of the phenomena observed, which embrace the whole system, both of the *motion* of the *electric fluid* in the *column*, and of the influence of the *ambient air* on this instrument, is obtained by suddenly removing the *rod*: for the new quantity of *electric fluid* communicated to the *column* by the *air* during the *influence* of the *negative* body makes both *gold leaves* strike at once on the *zinc* side by a very great *divergence*.

Effects of differences in the size & number of plates.

The last object concerning the theory of the *electric effects* in both the *column* and the *pile*, which remains to be considered, relates to the difference between the effects of the *number* of *groups* and the *size* of the *plates*, considered under two distinct points of view, on one of which I have already given an experiment in my former paper; but I have repeated it in a different manner, which will confirm my system on these phenomena.

Exp. 14.

Exp. 14. The *column* of 600 *groups*, represented in the *figure*, has been composed of three *columns* of 200 *groups* each, which I had used separately from the beginning of these experiments; but before they were united together for the purpose of the *aerial electroscope*, I tried their effects in the three following combinations.

1. I applied successively to the same *electroscope* the same extremity (either *positive* or *negative*,) of each *column*, the opposite extremity being placed in communication with the ground; and I observed the quantity of *divergence* produced by each *column*, which was nearly the same.

2. I applied the three *columns* at the same time to the *electroscope*, each of them remaining in communication with the ground; and the *divergence* was not greater than it had been with the most active of the single *columns*.

3. But having connected the three *columns* as one, by placing conductors between their opposite extremities, and connecting one extremity of the whole with the electroscope, the other being in communication with the ground; the effect was so much increased, that the *gold leaves* struck the sides of the electroscope.

Number of plates analogous to the length of a pump for raising water.

This proves, under a different form, the same proposition which I had stated in my former paper, namely, that the simple *divergence* in electroscopes depends only on the *density* of the *electric fluid*, and the *density* on the *number* of *groups*; at the same time that it confirms the cause which I had assigned to these effects; and as they are analogous to many kinds of phenomena, I shall use another example to explain it, that of *pumps*. As the *height* to which water can be raised by *pumps* does not depend on either their *number* or *size*, but on their *length*; so in the above experiment, with three concurrent *columns* of 200 groups, the *density* of the *electric fluid* was not increased on one extremity, nor consequently the *divergence* at either extremity, more than with one *column*; nor could more have been done with one column of the same *number* of *groups* of whatever *size*. But as, in taking the water at the same level, a *pump* of 30 feet will bring it three times as *high* as three *pumps* of 10 feet; so in the above experiment, the *column* of 600 *groups* produced probably three times as much change of *density* in the *electric fluid*, with a proportional *divergence*, as did the three *columns* of 200 groups, individually acting on the same low *level*, or degree of *density*, that of the *standard* of *plus* or *minus*.

and their size to that of the bore or number of *pumps*.

But if in this case the *size* of the *plates*, or the multiplication of their *number* at the same *numerical* distances from the extremities, be indifferent, it is not the same in some other cases, as I shall illustrate by the same analogy. In the above case, the *height* to which the *water* was to be raised being the only object, the number or size of the *pumps* was indifferent; but if a *current* is to be produced at that *level*, either with a certain degree of rapidity, or of a certain volume, then the *diameter* of the *pumps* comes in as a condition. The following experiment will show the analogy of this case, with the effects of the different *sizes* of *plates* in the *column*.

Exp. 15.

Exp. 15. I made two other *columns* of 200 groups each; but these I only cut square, for one of $\frac{1}{4}$, and for the other of $\frac{1}{8}$ of an inch, still *zinc* and *Dutch gilt paper*. These two *columns* produced sensibly the same *divergence* as the former, in the same electroscope; but in this was already shown the difference in other respects; the *time* for producing this *divergence* was in the inverse ratio of the *size* of the *plates*.

This experiment gives a clear idea of the effect produced by a greater *size* of the *plates*, both in the *pile* and in the *column*. In the *circuit* of the former, with the same *number* of *groups*, the effects are proportional to the *size* of the *plates*, because the *current* of the *electric fluid* becoming denser and more rapid in passing through the *wires* used in these operations, the effects are greater, in proportion to the number of *equal parts* of surface, either in a few or many *plates*, concurring to produce the *motion* of the *electric fluid* which arrives at the entrance of this narrow channel. That difference in the rapidity and density of the *current* cannot be discovered in the *circuit* of the *column*, because the condition to which *chemical effects* are owing is wanting in it, as I have explained in my former paper; but the *size* of the *plates* influences the *frequency* of the *strikings* of the little electroscopic *pendula*, when their simple *divergence* is exceeded; because each time that one of them *strikes*, either at the *negative* or the *positive* side, that instantaneous communication of the *column* with the ground changes in some degree its *electric* state; and the same state is sooner restored, to produce another *striking*, in proportion to the *size* of the *plates*, with the same *number* of *groups*. This effect will enter as an essential circumstance into the IId *part* of this paper, concerning the *aerial electroscope*.

II.

The Bakerian Lecture for 1809. On some new Electro-chemical Researches, on various Objects, particularly the metallic Bodies from the Alkalis, and Earths, and on some Combinations of Hidrogen, By HUMPHRY DAVY, *Esq. Sec. R. S. F. R. S. E. M. R. I. A.*

(Concluded from p. 55.)

IV. *On the Metals of Earths.*

Metals of the common earths

I HAVE tried a number of experiments, with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalis, and the alkaline earths.

apparently combined with iron.

I find, that, when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine, or glucine, slightly moistened and placed in hidrogen gas; the iron becomes brittle and whiter, and affords by solution in acids an earth of the same kind, as that which has been employed in the experiment.

Potassium in vapour

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

passed through silex,

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali formed upon the potassium. The potassium[*] was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour; so that there is great reason to believe, that the silex had been either entirely or partially deoxigenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

alumine, and glucine.

Potassium, in acting upon alumine and glucine, produced more hidrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable, that, even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks*, and leaving behind alkali and earth; and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small, that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them, which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

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[*] The results of this experiment are opposed to the idea, that potassium is a compound of hidrogen and potash, or its basis; for, if so, it might be expected, that the hidrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found, that the potassium produced as much hidrogen, as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

Attempt to form alloys with them.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect, that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance, which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury while the tube was hot; but the alkaline metal gave the characters to the amalgam: and though, in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied, that there was any of the metals of these earths in triple combination.

Mixtures of the earths, potassium, and iron filings formed alloys

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded by solution in acids,

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Homberg's pyrophorus.

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise.

acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Amalgams formed with the alkaline earths.

Though I could not procure decided evidences of the production of an amalgam from the metals of the common earths, yet I succeeded perfectly by the same method of operating in making amalgams of the alkaline earths.

Lime, and magnesia.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

Amalgam of magnesia.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder; and which, when acted upon by weak muriatic acid, gave off hidrogen gas in considerable quantities, and produced a solution of magnesia.

Proportions of metal in the earths may be obtained.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxigen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

Hypothetical calculation.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter; and that oxigen and inflammable bodies are in relations of attraction, which correspond to the functions of being negative and positive respectively; it would follow, that the attractions of acids for salifiable bases would be inversely as the quantity of oxigen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxigen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded, that barytes must contain the least oxigen of all the earths; and that the order, as to the quantity of inflammable matter, must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxigen of all.

Inferences.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent, strontites 86†, lime 73·5*, magnesia, 66‡.

Composition of protoxides and neutral salts.

The same proportions would follow from an application of Mr. Dalton's ingenious supposition§, that the proportion of oxigen is the same in all protoxides; and that the quantity of acid is the same in all neutral salts, i. e. that every neutral salt is composed of one particle of metal, one of oxigen, and one of acid.

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* Mr James Thompson, Nicholson's Journal, vol. xxiii, p. 175, and Berthier.

† Mr. Clayfield. Thompson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol iii, p. 616.

§ The principle, that I have stated, of the affinity of an acid for a salifiable basis being inversely as the quantity of oxigen contained by the basis, though gained from the comparison of the electrical relations of the earths with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalis and alkaline earths; which, in the case of the alkalis, were not very remote from what I had ascertained by direct experiments. Mr. Gay-Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxigen, might (as far as it is correct) be inferred from Mr. Dalton's law; though this ingenious chemist states, that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxigen, the same in all protoxides; and there is a proportion of acid, the same in all neutral salts; and new proportions of oxigen and of acid are always multiples of these proportions. So that, if a protoxide, in becoming a deutoxide, takes up more acid, it will be at least double the quantity; and in these cases the oxigen will be strictly as the acid. Mr. Dalton's law even provides for cases, to which Mr. Gay-Lussac's will not apply; a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxisulphat of iron perfectly formed, (as some experiments, which I have lately made, seem to show,) there is probably only a single proportion of acid; and in the supertartrite of potash there is only a single quantity of oxigen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

Proportion of metal in alumine,

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phosphate of alumine, alumine would appear to contain about 56 per cent of metallic matter.

and silex.

Mr. Berzelius, in a letter which I received from him a few months ago†, states, that, in making an analysis of cast iron, he found, that it contained the metal of silex; and that this metal, in being oxidated, took up nearly half its weight of oxigen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxigen, which agrees very nearly with the quantity of hidrogen and ammonia produced from the amalgam.

Earths formerly considered of the same class with oxides: afterwards alkalis, earths, and oxides, deemed separate orders,

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston; and though Lavoisier insisted upon this analogy with his usual sagacity; yet still the alkalis, earths, and oxides, have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls§; and the alkalis and alkaline earths are both distinguished by their solubility in water: but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalis and alkaline earths combine

but apparently without reason.

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* Thomson's Chemistry, vol. ii, p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of Mr. Gay-Lussac, on the combinations of gaseous bodies, Mem. d'Arcueil, tom. ii, page 213; and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by Messrs. Gay-Lussac, and Thenard.

§ Klaproth. Annales de Chimie, tom. x, p. 277.

with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and Mr. Guyton,) precipitate the triple prussiate of potash: the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutrosaline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalis and earths with oils, to form soaps; and of the earthy soaps some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxigen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalis approach to the lightest class of oxidable metals?

It will be unnecessary, I trust, to pursue these analogies any farther; and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

Alloys formed in metallurgical operations, which affect the qualities of the metals; as iron,

It is probable, that these alloys may be formed in many metallurgical operations; and that small quantities of them may influence materially the properties of the compound, in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold short iron; while, from another spot, it will make hot short: but by a combination of the two in due proportions, good iron is produced. May not this be owing to the circumstance of their containing different metals of the earths, which in compound alloys may be more oxidable than in simple alloys, and may be more easily separated by combustion?

and copper.

Copper, Mr. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Speculations on the nature of hidrogen,

Hidrogen is the body which combines with the largest proportion of oxigen, and yet it forms with it a neutral compound. This on the hypothesis of electrical energy would show, that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxigen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hidrogen are of the same kind, and lead to similar conclusions.

muriatic acid gas,

Muriatic acid gas, as I have shown, and as is farther proved by the researches of Messrs. Gay-Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decomposed, unless a new combination is formed: thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decomposed by all the metals; and in these cases hidrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxigen being found in the new compound. This at first view might be supposed in favour of the idea, that hidrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxigenation.

and nitrogen.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia; and (even supposing no reference to be made to the experiments detailed in this paper,) the general analogy of chemistry would lead to the notion of its being compounded.

Are hidrogen, ammonia, and nitrogen, oxides of the same metal?

Should it be established by future researches, that hidrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal; the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus*, and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter to acid matter might be supposed capable of being ascertained by their relations to oxigen, or by the peculiar state of their electrical energy.

Sulphur and phosphorus appear to contain hidrogen.

* The electrization of sulphur and phosphorus goes far to prove, that they contain combined hidrogen. From the phenomena of the action of potassium upon them in my first experiments I conceived, that they contained oxigen; though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxigen; till I discovered, that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hidrogen, and on phosphuretted hidrogen, I find that the phenomena differ very much according to the circumstances of the experiment; and in some instances I have obtained a larger volume of gas from potassium, after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxigen in sulphur and phosphorus is however still supported by various analogies. Their being nonconducters of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hidrogen, mixed with a small quantity of atmospheric air, absorb both oxigen and hidrogen, and become nonconducting inflammable bodies analogous to resinous and oily substances.

The whole tenour of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

Phlogistic hypothesis.

If hidrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxigen, hidrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases with hidrogen; the earths, the fixed alkalis, metallic oxides, and the common acids, will be compounds of the same bases, with water.

Arguments in favour of this idea.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are, First, The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hidrogen or oxigen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hidrogen, whether in its simple form, or in combination with one proportion of oxigen, or a double proportion, from my experiments, seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hidruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second. The facility with which metallic substances are revived, in cases in which hidrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of 6 inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed. The difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hidrogen is essential to the production of the metal.

3d.

Third, Oxigen and hidrogen are bodies, that in all cases seem to neutralize each other; and therefore, in the products of combustion, it might be expected, that the natural energies of the bases would be most distinctly displayed, which is the case; and in oximuriatic acid, the acid energy seems to be blunted by oxigen, and is restored by the addition of hidrogen.

Arguments against it.

In the action of potassium and sodium upon ammonia, though the quantity of hidrogen evolved in my experiments is not exactly the same, as that produced by their action upon water; yet it is probable, that this is caused by the imperfection of the process*: and supposing potassium and sodium to produce the same quantity of hidrogen from ammonia and water, the circumstance at first view may be conceived favourable to the notion, that they contain hidrogen, which, under common circumstances of combination, will be repellent to matter of the same kind; but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for, on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then, whenever bodies known to contain hidrogen are decomposed by a metal, the quantities of hidrogen ought to be the same, or multiples of each other. Thus in the decomposition of ammonia by potassium and sodium, two of hidrogen and one of nitrogen remain in combination, and one of hidrogen is given off; and in the action of water on potassium to form potash, the same quantity of hidrogen ought

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[*] There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hidrogen evolved; i. e. whenever the metals of the alkalis act upon ammonia, (supposing this body to be composed of 3 hidrogen, and 1 of nitrogen, in volume, 2 of hidrogen and 1 of nitrogen remain in combination, and 1 of hidrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalis and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

to be expelled. From my analysis* of sulphuretted hydrogen it would appear, that, if potassium in forming a combination with this substance sets free hidrogen, it will be nearly the same quantity, as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett of the sulphuret of iron be made a basis of calculation, iron, in attracting sulphur from sulphuretted hidrogen, will liberate the same proportion of hidrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hidrogen, because in acting upon different combinations containing hidrogen they produce the evolution of equal proportions of this gas, then it might be proved, that almost any kind of matter is contained in any other. The same

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Composition of sulphuretted hidrogen.

* The composition may be deduced from the experiments in the last Bakerian lecture, which show, that it contains a volume of hidrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hidrogen, and 32·73 of sulphur. When sulphuretted hidrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hidrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hidrogen, it was found, that, when these gasses were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common states; the phosphorus was dark coloured, and the arsenic appeared as a brown powder: both were probably hydrurets. This is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hidrogen: when the metal is in smaller quantity than is sufficient to decompose the whole of the gasses, there is always an expansion of volume; so that arseniuretted and phosphuretted hidrogen contain in equal volumes more hidrogen than sulphuretted hidrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hidrogen, it would appear, that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals, that I have experimented upon, in acting upon muriatic acid gas evolve the same quantity of hidrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The nature of the amalgam of ammonia of importance in deciding the question.

The solution of the general question concerning the presence of hidrogen in all inflammable bodies will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxigen, which may be supposed to exist in hidrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hidrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present occupy the time of the Society, by entering any farther into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined, to enable us to decide upon their nature, and their relations, or to form any general theory concerning them, which is likely to be permanent.

III.

The Croonian Lecture. On the Functions of the Heart and Arteries. By THOMAS YOUNG, *M. D. For. Sec. R. S.*

(Concluded from p. 68.)

Functions of the muscular fibres of the coats of the arteries.

They have less effect on the motion of the blood, than is generally supposed.

Arguments to prove this,

I Shall proceed to inquire, in the third place, into the nature and extent of the functions, which are to be attributed to the muscular fibres of the coats of the arteries; and I apprehend, that it will appear to be demonstrable, that they are much less concerned in the progressive motion of the blood, than is almost universally believed. The arguments, which may be employed to prove this, are nearly the same that I have already stated, in examining the motion of a fluid, carried along before a moving body in an open canal; but in the case of an elastic tube, the velocity of the transmission of an impulse being rather diminished than increased by an increase of tension, the reasoning is still stronger and simpler; for it may here be safely asserted, that the anterior parts of the dilatation, which must be forced along by any progressive contraction of the tube, can only advance with the velocity appropriate to the tube, and that its capacity must be proportionate to its length and to the area of its section: now the magnitude of its section must be limited by that degree of tension, which is sufficient to force back through the contraction what remains of the displaced fluid; and the length, by the difference of the velocity appropriate to the tube, and that with which the contraction advances: consequently, if the contraction advance with the velocity of a pulsation, as any contractile action of the arteries must be supposed to do, this length necessarily vanishes, and with it the quantity of the fluid protruded; the whole being forced backwards, by the distending force which is exerted by a very small dilated portion immediately preceding the contraction. It might indeed be imagined, that the contraction follows the pulsation with a velocity somewhat smaller than its own; but this opinion would stand on no other foundation than mere

Supposition, that the contraction follows the pulsation with less velocity;

mere conjecture, and it would follow, that the pulse would always become more and more full, as it became more distant from the heart; of which we have nothing like evidence: nor would a moderate contraction, even if this supposition were granted, produce any material effect. For example, if the velocity of the contraction were only half as great as that of the pulsation, which is the most favourable proportion, it would be necessary, taking sixteen feet in a second for the velocity of the pulsation, that the section of the arteries should be contracted to about one half, in order to produce, by their progressive contraction only, the actual velocity of the blood in the aorta; one sixteenth of the blood being carried, in this case, before the contraction: but if the contraction were only such, as to reduce the section of the artery to $\frac{9}{10}$, which is probably more than ever actually happens, the velocity produced would be only about $\frac{1}{80}$ as much; and if the contraction were only to $\frac{99}{100}$, which is a sufficient allowance for the smaller arteries, about $\frac{1}{1000}$ only of the actual velocity in the aorta could be produced in this manner, even upon a supposition much more favourable to the muscular action of the arteries than the actual circumstances. A small addition must be made to the force required for producing the retrograde motion, on account of the friction to be overcome, but the general reasoning is not affected by this correction.

or that the contraction remains after each pulsation,

The contraction of the artery might also be supposed to remain after each pulsation, so that the vessel should not be again dilated until the next pulsation, or, in other words, a spontaneous dilatation might be supposed to accompany the pulsation, instead of a contraction: but such a dilatation would be useless in promoting the progressive motion of the blood, since a larger quantity of blood, conveyed to the smaller vessels, without an increased tension, would be ineffectual with respect to the resistances which are to be overcome. It is possible indeed, that the muscular fibres of those arteries in which the magnitude of the pulse is sensible, like the fibres of the heart, may be inactive, or nearly so, during their dilatation; and that they may contract, after they have been once distended, with a force which is in a certain degree permanent; the greater momentum of the blood, which accompanies the dilatation, enabling it to enter the minute arteries with equal ease, although assisted by a tension somewhat smaller: so that the same mean velocity may be sustained, as if the arteries were simply elastic, and a little smaller in diameter, with a very little less exertion of the heart. But the distribution of the blood could never be materially diversified by any operation of this kind: for if any artery were for a moment distended by such a variation, so as to exceed its natural diameter by one hundredth part only, a pressure would thence arise equivalent to that of a column about two inches high, which would, in spite of all resistances, immediately dissipate the blood with a considerable velocity, and completely prevent any local accumulation, unless the elastic powers of the vessel itself were diminished; and this is, perhaps, the most important, as well as the best established inference from the doctrine that I have advanced.

But the distribution of the blood not materially affected in this way.

So that there can be no local accumulation, if the elasticity of the vessels be not diminished.

Circulation in a mola without a heart;

It appears, that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries: in this case it cannot be ascertained, whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance in the extreme vessels being also probably much smaller than usual: if the motion was continued, it would lead us to imagine, that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants, by which they are calculated to promote the ascent of the sap. The circulation in the vessels of the more imperfect animals, in which a great artery supplies the place of a heart, is of a very different nature from that of the more perfect animals: the great artery, which performs the office of the heart, is here possessed of a muscular power commensurate to its functions, and seems to propel the blood, though much more slowly than in other cases, by means

and in animals without a heart.

means of a true peristaltic motion. It appears also from the observations of Spallanzani, that in many animals a portion of the aorta, next the heart, is capable of exhibiting a continued pulsation, even when perfectly empty and separated from the heart; but this property is limited to a small part of the artery only, which is obviously capable of being essentially useful in propelling the blood, when the valves of the aorta are closed. The muscular power of the termination of the vena cava is also capable of assisting the passage of the blood into the auricle. It is not at all improbable, that a muscle of involuntary motion, which had been affected throughout the whole period of life by alternate contractions and relaxations, might retain from habit the tendency to such contractions, even without the necessity of supposing, that the habit was originally formed for a purpose to be obtained by the immediate exertion of the muscular power; but in fact the partial pulsation of the vena cava is perfectly well calculated to promote the temporary repletion of the auricle, while it must retard, for a moment, the column which is approaching, at a time that it could not be received.

apparently by a peristaltic motion.

In many animals part of the aorta capable of pulsation.

Muscular power of the end of the vena cava.

Use of its partial pulsation.

There is no difficulty in imagining what services the muscular coats of the arteries may be capable of performing, without attributing to them any immediate concern in supporting the circulation. For since the quantity of blood in the system is on many accounts perpetually varying, there must be some means of accommodating the blood vessels to their contents. This circumstance was very evident in some of Hales's experiments, when, after a certain quantity of blood had been taken away, the height of the column, which measured the tension of the vessels, frequently varied in an irregular manner, before it became stationary at a height proportional to the remaining permanent tension. Haller also relates, that he has frequently seen the arteries completely empty, although in some of his observations there was probably only a want of red globules in the blood which was flowing through them. Such alterations in the capacity of the different parts of the body are almost always to be attributed to the exertion of a muscular power. A partial contraction of the coats of the smaller

Services to which the muscular coats of the arteries are adapted.

arteries may also have an immediate effect on the quantity of blood contained in any part, although very little variation could be produced in this manner by a change of the capacity of the larger vessels.

The state of the pulse depends almost wholly on the action of the heart.

According to this statement of the powers which are concerned in the circulation, it must be obvious, that the nature of the pulse, as perceptible to the touch, must depend almost entirely on the action of the heart, since the state of the arteries can produce very little alteration in its qualities.

Modifications of it by the state of the artery.

The greater or less tension of the arterial system may indeed render the artery itself, when at rest, somewhat harder or softer; and, if the longitudinal fibres give way to the distending force, it may become also tortuous: possibly too a very delicate touch may in some cases perceive a difference in the degree of dilatation, although it is seldom practicable to distinguish the artery, in its quiescent state, from the surrounding parts. But the sensation, which is perceived when the artery is compressed, as usual, by the finger, is by no means to be confounded with the dilatation of the artery; for in this case an obstacle is opposed to the motion of the blood, against which it strikes, with the momentum of a considerable column, almost in the same manner as a stream of water strikes on the valve of the hydraulic ram; and in this manner, neglecting the difference of force arising from the different magnitudes of the sections, the pressure felt by the finger becomes nearly equal and similar to that which is originally exerted by the heart: each pulsation passing under the finger, in the same time, as is required for the contraction of the heart, although a very little later, and more or less so, in proportion as the artery is more or less distant; the artery remaining then at rest for a time equal to that in which the heart is at rest. When therefore an artery appears to throb, or to beat more strongly than usual, the circumstance is only to be explained from its greater dilatation, which allows it to receive a greater portion of the action of the heart, in the same manner as an aneurism exhibits a very strong pulsation, without any increase of energy, either in itself, or in the neighbouring vessels; and on the other hand, when the pulsations of the artery of a paralytic arm become feeble, we cannot hesitate to attribute the change to its permanent contraction, since the enlargement and contraction of the blood vessels of a limb are well known to attend the increase or diminution of its muscular exertions. There is also another way, in which the diminution of the strength of an artery may increase the apparent magnitude of the pulse, that is, by diminishing the velocity with which the pulsation is transmitted: for we have seen, that the magnitude of the pulse is in the inverse ratio of the length of the artery distended at once; and this length is proportional to the velocity of the transmission: but it must be observed, that the force of the pulse striking the finger would not be affected by such a change, except that it might be rendered somewhat fuller and softer, although a considerable throbbing might be felt in the part, from the increased distension of the temporary diameter of the artery. How little a muscular force is necessary for the simple transmission of a pulsation may easily be shown, by placing a finger on the vena saphena, and striking it with the other hand at a distant part; a sensation will then be felt precisely like that of a weak arterial pulsation.

Muscular force not required to transmit a pulsation.

The deviations from the natural state of the circulation, which are now to be cursorily investigated, may be either general or partial; and the general deviations may consist either in a change of the motion of the heart, or of the capacity of the capillary arteries. When the motion of the heart is affected, the quantity of blood transmitted by it may either remain the same as in perfect health, or be diminished, or increased. Supposing it to remain the same, the pulse, if more frequent, must be weaker, and if slower, it must be stronger; but this latter combination is scarcely ever observable; and in the former case, the heart must either never be filled, perhaps on account of too great irritability, or never be emptied, from the weakness of its muscular powers. But the immediate effect of such a change as this, in the functions depending on the circulation, cannot be very material, and it can only be considered as an indication of a derangement in the nervous and muscular system, which is not likely to lead to any disease of the vital functions. When the quantity of blood transmitted by the heart

Deviations from the natural state of the circulation,

when the motion of the heart is affected.

heart is smaller than in health, the arteries must be contracted, until their tension becomes only adequate to propel the blood, through the capillary vessels, with a proportionally smaller velocity, and the veins must of course become distended, unless the muscular coats of the arteries can be sufficiently relaxed to afford a diminished tension, which is probably possible in a very limited degree only. In this state the pulse must be small and weak, and the arteries being partly exhausted, there will probably be a paleness and chilliness of the extremities; until the blood, which is accumulated in the veins, has sufficient power to urge the heart to a greater action, and perhaps, from the vigour which it may have acquired during the remission of its exertions, even to a morbid excess of activity. Hence a contrary state may arise, in which the quantity of blood transmitted by the heart is greater than in perfect health; the pulse will then be full and strong, the arteries being distended, so as to be capable of exerting a pressure sufficient to maintain an increased velocity, and to overcome the consequent increase of resistance; a state which perhaps constitutes the hot fit of fever; and which is probably sometimes removed in consequence of a relaxation of the extreme arteries, which suffer the superfluous blood to pass more easily into the veins. Such a relaxation, when carried to a morbid extent, may also be a principal cause of another general derangement of the circulation, the motion of the blood being accelerated, and the arteries emptied, so that the pulse may be small and weak, while the veins are overcharged, and the heart exhausted by violent and fruitless efforts to restore the equilibrium; and this state appears to resemble, in many respects, the affections observed in typhus. When, on the contrary, the capillary vessels are contracted, the arteries are again distended, although without the excess of heat which must attend their distension from an increased action of the heart, and possibly without fever; an instance of this appears to be exhibited in the shrinking of the skin, which is frequently observable from the effect of cold, and in the first impression produced by a cold bath; nor is it impossible, that such a contraction may exist in the cold fit of an intermittent, although it seems

Hot fit of fever.

Typhus.

Effect of cold.

Cold fit of fever.

more probable, that a debility of the heart is the primary cause of this affection.

Local inflammations.

Partial affections of the capillary vessels.

Inflammation.

Perhaps the properties of the blood ultimately altered.

Beside these general causes of derangement, which appear to be more or less concerned in different kinds of fever, there are other more partial ones, which seem to have a similar relation to local inflammations. The most obvious of these changes are such as must be produced by partial dilatations or contractions of the capillary vessels; since, as I have endeavoured to demonstrate, any supposed derangement in the actions of the larger vessels must be excluded from the number of causes which can materially affect the circulation. It cannot be denied, that a diminution of the elastic, or even of the muscular force of the small arteries, must be immediately followed by such a distension, as will produce a resistance equal to the pressure: the distension will occasion an increase of redness, and in most cases pain: the heat will also generally be increased, on account of the increased quantity of blood, which will be allowed to pass through the part; and since the hydrostatic pressure of the blood acquires greater force, as the artery becomes more distended, it may be so weak as to continue to give way, like a ligament which has been strained, until supported by the surrounding parts. In this state a larger supply of blood will be ready for any purposes which require it, whether an injury is to be repaired, or a new substance formed; and it is not impossible, that this change in the state of the minute vessels may ultimately produce some change in the properties of the blood itself.

The more the capillary arteries are debilitated and distended, the greater will be the mean velocity of the circulation; but whether or no the velocity will be increased in the vessels which are thus distended, must depend on the extent of the affected part; and it may frequently happen, that the velocity may be much more diminished on account of the dilatation of the space which the blood is to occupy, than increased by the diminution of the resistance. And on the other hand, the velocity may be often increased, for a similar reason, at the place of a partial contraction. Hence we may easily understand some of the experiments, which Dr. Wilson has related in his valuable treatise on fevers: the

Experiments in Wilson on Fevers.

the application of spirit of wine to a part of the membrane of a frog's foot contracted the capillary arteries, and at the same time accelerated the motion of the blood in them, while in other parts, where inflammation was present, and the vessels were distended, the motion of the blood was slower than usual.

Another species of inflammation.

Another species of inflammation may probably be occasioned by a partial constriction or obstruction of the capillary arteries, which must indeed be supposed to exist where the blood has become wholly stagnant, as Dr. Wilson in some instances found it. This obstruction must however be extended to almost all the branches, belonging to some small trunk, in which the pressure remains nearly equal to the tension of the large arteries; for in this case it will happen, that the whole pressure will be continued throughout the obstructed branches, without the subtraction of the most considerable part, which is usually expended in overcoming the resistances dependent on the velocity; so that the small branches will be subjected to a pressure, many times greater than that which they are intended to withstand in the natural state of the circulation; whence it may easily happen, that they may be morbidly distended; and this distension may constitute an inflammation, attended by redness and pain. Nor is it impossible, that obstructions of this kind may originate in a vitiated state of the blood itself, although it would be difficult to prove the truth of the conjecture; it seems, however, to be favoured by the observation of Haller, that little clots of globules may often be observed in the arteries, when the circulation is languid, and that they disappear when its vigour is restored, especially after venesection. But if a very small number only of capillary arteries be obstructed, other minute branches will still be capable of receiving the blood, which ought to pass through them, without any great distension or increase of pressure: and this exception is sufficient to explain another experiment of Dr. Wilson, in which a small obstruction, caused by puncturing a membrane with a hot needle, failed to excite an inflammation. This species of inflammation is probably attended by less heat than the former; and where the obstruction is very great, it may perhaps lead immediately to a mortification, which is called by the Germans "a cold burning."

These conjectures consistent with the common causes of inflammation.

The most usual causes of inflammation appear to be easily reconcilable with these conjectures. Suppose any considerable part of the body to be affected by cold; the capillary vessels will be contracted, and at the same time the temperature of some parts of their contents will be lowered, from both of which causes the resistance will be increased, and the arteries in general will be more or less overcharged: if then any other part of the system be at the same time debilitated or overheated, its arteries will be liable to be morbidly distended, and an inflammation may thus arise, which may continue till the minute vessels are supported and strengthened, by means of an effusion of coagulable lymph. The immediate effect, either of cold or of heat, may also sometimes produce such a degree of debility in any part, as may lay the foundation of a subsequent inflammation: but the first effect of heat in the blood-vessels appears to be the more ready transmission of the blood into the veins, by means of which they become very observably prominent: and cold, which checks the circulation in the cutaneous vessels, probably occasions a livid hue, by retaining the blood stagnant longer than usual in the capillary vessels of all kinds. It may be objected, that an obstruction of the motion of the blood through a great artery ought, upon these principles, to produce an inflammation in some distant part: but in this case, the blood will still find its way very copiously into the parts supplied by the artery, by means of some collateral branches, which will always admit a much larger quantity of blood than usually passes through them, whenever a very slight excess of force can be found to carry it on, or when the blood which they contain finds a readier passage than usual, by means of their communication with such parts as are now deprived of their natural supply.

Blushing.

It is difficult to determine, whether blushing is more probably effected by a constriction or by a relaxation of the vessels concerned; it must, however, be chiefly an affection of the smaller vessels, since the larger ones do not contain a sufficient quantity of blood to produce so sudden an effect.

Perhaps the capillary vessels are dilated, while the arteries, which are a little larger only, are contracted: possibly too an obstruction may exist at the point of junction of the arteries with the veins; and where the blush is preceded by paleness, such an obstruction is probably the principal cause of the whole affection.

Tendency of inflammation to extend itself.

With respect to the tendency of inflammation in general to extend itself to the neighbouring parts, it is scarcely possible to form any reasonable conjecture, that can lead to its explanation: this circumstance appears to be placed beyond the reach of any mechanical theory, and to belong rather to some mutual communication of the functions of the nervous system; since it is not inflammation only, that is thus propagated, but a variety of other local affections of a specific nature, which are usually complicated with inflammation, although they may perhaps, in some cases, be independent of it. Inflammations, however, are certainly capable of great diversity in their nature, and it is not to be expected, that any mechanical theory can do more than to afford a probable explanation of the most material circumstances, which are common to all the different species.

Operation of remedies for inflammation and fevers.

Beside these general illustrations of the nature of fevers and inflammations, the theory which has been explained may sometimes be of use, in enabling us to understand the operation of the remedies employed for relieving them. Thus it may be shown, that any diminution of the tension of the arterial system must be propagated from the point at which it begins, as from a centre, nearly in the same manner, and with the same velocity, as an increase of tension, or a pulsation of any kind would be propagated. Hence the effect of venesection must be not only more rapidly, but also more powerfully felt in a neighbouring than in a distant part: and although the mean or permanent tension of the vessels of any part must be the same, from whatever vein the blood may have been drawn, provided that they undergo no local alteration, yet the temporary change, produced by opening a vein in their neighbourhood, may have relieved them so effectually from an excess of pressure, as to allow them to recover their natural tone, which they could not have done without such a partial exhaustion of the

Topical venesection.

the neighbouring vessels. But since it seems probable, that the minute arteries are more affected by distension than the veins, there is reason in general to expect a more speedy and efficacious relief in inflammations, from opening an artery than a vein: this operation, however, can seldom be performed without material inconvenience; but it is probably for a similar reason, that greater benefit is often experienced from withdrawing a small portion of blood by means of cupping or of leeches, than a much larger quantity by venesection, since both the former modes of bleeding tend to relieve the arteries, as immediately as the veins, from that distension, which appears to constitute the most essential characteristic of inflammation. In a case of hemorrhage from one of the sinuses of the brain, a very judicious physician lately prescribed the digitalis: if the effect of this medicine tends principally to diminish the action of the heart, as is commonly supposed, it was more likely to be injurious than beneficial, since a venous plethora must be increased by the inactivity of the heart; but if the digitalis diminishes the general tension of the arteries, in a greater proportion than it affects the motion of the heart, it may possibly be advantageous in venous hemorrhages. We have, however, no sufficient authority for believing, that it has any such effect on the arterial system in general.

Arteriotomy.

Cupping and leeches.

Use of foxglove in hemorrhage.

Although the arguments, which I have advanced, appear to me sufficient to prove, that, in the ordinary state of the circulation, the muscular powers of the arteries have very little effect in propelling the blood, yet I neither expect nor desire, that the prevailing opinion should at once be universally abandoned. I wish, however, to protest once more against a hasty rejection of my theory, from a superficial consideration of cases, like that which has been related by Dr. Clarke; and to observe again, that the objections, which I have adduced, against the operation of the muscular powers of the arteries in the ordinary circulation, not being applicable to these cases, they are by no means weakened by any inferences which can be drawn from them.

Muscular powers of the arteries have little effect on the ordinary circulation.

IV.

Deſcription of a Scarificator on a new Principle. By Mr. THOMAS SHUTE, *Surgeon.*

SIR,

New scarificator.

IF the annexed description of a Scarificator, which I have found upon trial to be extremely efficient, should appear worthy of insertion in your Journal, I have taken the liberty of transmitting it to you for that purpose.

I am, Sir,
Your most obedient humble Servant,
Park Street, Bristol, THOMAS SHUTE, *Surgeon.*
22d July, 1810.

Cupping evidently advantageous.

The operation sometimes tedious, painful, and ineffectual,

probably from defects of the instrument.

Alteration of its principle.

The advantages resulting from a local evacuation of blood by cupping, in a variety of complaints, being fully established, it would, I presume, be a waste of time elaborately to descant on the merits of such depletion, as forming a high and important remedy in the curative art. It must however be admitted, that the operative means, which have been hitherto employed for this purpose, are not only too often tedious and painful in their applications, but very frequently extremely ineffectual in the event. Such being the acknowledged fact, and regarding it as very improbable, that the difficulty of obtaining blood could depend on a want of manual dexterity in the operator, when the scarificator usually employed had passed through the hands of so many able practitioners, it seemed natural to conclude, that the want of success ought rather to be attributed to some fault in the construction of the instrument itself. Impressed with these ideas, and having taken up an opinion, that the failure of the scarificator now in use might be attributed to the manner in which the incisions are made; and supposing, that simple punctures would more certainly enter the depths intended; I flatter myself, that, by altering the principle on which the instrument used to act, I have produced one, which will effect all the purposes required with more facility to the operator, and less pain to the patient.

Without any intention then extravagantly to extol the properties of a new instrument, or unnecessarily to depreciate the merits of an old one, I take the liberty of recommending one to my medical brethren for their approbation, which I have found to answer in my hands much better than any other that I have yet been able to procure. That the instrument here recommended will invariably produce the wished for effect, I am sanguine enough to believe; at the same time that I, by no means, mean to assert it is not still capable of farther improvement.

This instrument effectual.

A draught taken by Mr. Mac Donald, a friend and pupil of mine, is subjoined, sufficiently explanatory as I hope of the fabric of the instrument, which may be purchased of Mr. Winter, Cutler, Bridge Street.

It is my intention, at no very distant period, to offer a few observations on the formation and number of the lancets, so as more immediately to adapt them to particular parts of the body.

Explanation of the Plate.

Plate IV, fig. 1, 2, and 3. *a* a nut, by means of which, acting on the screw *b*, the plates *c* and *d*, holding the lancets, are drawn upwards, till the catch *e* falls into the notch at *f*. The nut is then unscrewed; and, by pushing in the knob *g*, the catch is withdrawn, and the worm spring *h* immediately forces down the lancets.

The scarificator described.

i, a spring acting on the catch *e*, to force it into the notch.

k, a box, which, by means of the screws *l l*, regulates at will the exposure of the lancets, and in consequence the depth of the incisions.

The figures are on a scale of half an inch to an inch.

V.

On the Theory of Capillary Attraction. By THOMAS KNIGHT, *Esq.* In a Letter from the author.

To Mr. NICHOLSON.

SIR,

Theory of capillary action imperfect.

NOTWITHSTANDING the attention, that has lately been paid to the phenomena arising from capillary action, the theory appears to be still in a very imperfect state. I have endeavoured, in what follows, to throw some light on that part, which is most defective. The insertion of it, in your valuable Journal, will speedily bring my ideas before competent judges,

And much oblige, Sir,
Your most obedient Servant,
Papcastle, near Cockermouth, THOMAS KNIGHT.
July 22d, 1810.

The phenomena may be estimated from the figure of the surface, or from the forces.

There are two ways of treating the subject of capillary action; first, the measure of the *other* phenomena may be estimated from the figure of the surface, which is a *simultaneous effect* with the height of the fluid: or, secondly, in a more natural manner, from the forces themselves which support the fluid.

Mistake of Mr. la Place.

If, with Mr. La Place and Dr. Young, the first method be made use of, we must take care not to mistake an effect for the cause, as the former of these authors appears *at first* to have done*. But, even if we do not fall into this errour, a theory, which stops here, must appear, I think, to any one, to be exceedingly defective. Let the hydrostatic principle, of the perpendicularity of the force, to the surface, be used to explain what relates to the figure of that surface:

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* It is very remarkable, that Dr. Young, in his observations on Mr. la Place's theory, should not have noticed the chief circumstance in which it differs from all others: viz. "Que l'eau s'élève, dans un tube capillaire, *par l'effet* de la concavité de sa surface intérieure." Sur l'Action capillaire p. 60. Whether Dr. Young himself be of this opinion, I do not very clearly perceive.

but, by all means, let us have a view of *the mechanism by which the whole column is supported.*

Errour in his second method.

Mr. La Place, probably from considering the matter in this light, gave the world his *second* method; which appears to be as erroneous in falsely explaining the true*, as the *first* had been in assigning a wrong cause.

Object of the author.

My own intention, at present, is to show what part of a capillary tube keeps a fluid elevated; and the precise manner, in which it causes this elevation: or, in other words, to supersede this second method of Mr. La Place, and all similar theories, by one more conformable to truth.

Experiment of Abat.

The remarkable experiment of Abat, which Mr. La Place seems to have thought one of the best proofs of his first theory, will be very easily explained here on quite different principles.

We shall also see what is *the limit* of the height of the fluid in that experiment, which no one I believe has yet shown.

Attraction of the fluid for itself and for the tube differs only in intensity.

I suppose, with Mr. La Place, and other writers on the subject, that the attractions of the particles of the fluid for itself, and of the tube for the fluid, extend only to insensible distances; that they follow the same law of the distances; and only differ by their intensity at the same distance.

Prop.

Proposition.

Let A B C D E F (fig. 4) be the section, through the axis, of a circular tube, every where of equal diameter, bent into a rectangular form, and standing in a vertical plane. Let the part A B C D, of the tube, be formed of matter, the intensity of attraction of which for the fluid within it is represented by r, while that of the other part C D E F is r'. The excess of the mass of fluid in the leg A B over that in the leg E F is as $(2r-2r') \times$ diameter of the tube.

Demonstration.

Let *e n m n e* be a slender canal of fluid, extending from the surface in one leg to that in the other, and parallel to the side of the tube, as well as every where at the same distance from it. It is, in the first place, evident that this canal is not urged either way, except by those parts of the tube which are situate near the surface of the fluid.

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* The balancing force of the tube.

For, from *l*, any particle of the tube, set off *l m*, *l n*, equal to one another, and of any length less than the radius of the sphere of action of the particle.

If this particle urges the canal in one direction by its action at *m*, it urges it equally in the contrary direction by its action at *n**.

We will now see what action the canal sustains near the surface *c e d*. With a radius *e f*, equal to that of the sphere of action of the particles, and with the point *e* for a centre, describe the circular arc *o f p*.

The canal *e r* is urged upwards by the resolved action of those particles of the section of the tube contained in the space *o f p d o*; and if this space be divided into two equal parts, by the horizontal line *c f*, the action of the part above this line draws the canal as much upwards as that of the lower part does. For from any point *g*, in the lower part, draw *g e*, *g h* equal to one another; the action of *g* on the part *e h* urges the canal neither upwards nor downwards; for its action on any point above *h* is counteracted by its action on another point at the same distance below *e*. But there are particles below *h*, and within the sphere of action of *g*, as at *k*, on which it exercises an unbalanced action tending to draw the canal upwards. Next, suppose *i*, in the upper part of the space *o f p d o*, similarly situate to *g* in the lower. Draw *e i*, which will be parallel to *g h*, and will evidently draw upwards that part of the canal below *e*, as much as *g* draws upwards the part below *h*.

The other end *ε ϱ*, of the canal *e n m ε*, is urged upwards, in like manner, by the action of the space $\omega\ \varphi\ \varpi\ \delta\ \omega$, equal to, and described in the same manner as, $o\, f\, p\ d\ o$.

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* This is so plain, that one is astonished to find Mr. la Place, in his second method, making the chief part of the force, which elevates the fluid, reside at the junction of the two tubes. See "*Supplément a la Théorie de l'Action Capillaire*," p. 16. Clairaut also, in a theory, to which it has been lately the fashion to give very undeserved praise, falls into the same errour. "*Figure de la Terre*," p. 119. The false proposition, that a mass with a plane surface presses a slender column within it *downwards*, to which most of Mr. la Place's errours may be traced, has Clairaut for its original author. Haüy has the same figure, and nearly the same words.

Now, suppose both sides of the section of the tube to revolve round the axes $a\ b$, $\alpha\ \beta$; the cylindrical annulus generated by $e\ r$ is urged upwards by the action of the annulus generated by the circular space $o\, f\, p\ d\ o$; and that generated by $\iota\ \varrho$ by that generated by $\omega\ \varphi\ \pi\ \delta\ \omega$. If we represent the action of the annulus generated by $o\, f\, p\ d\ o$ by $2\ r$, that of the annulus generated by $\varpi\ \varphi\ \pi\ \delta\ \omega$ will be $2\ r'$.

By reasoning in the same manner with respect to all other cylindrical annuli within the sphere of action of the tube, and taking the sum of all the actions, it is plain, that the excess of the mass of the fluid in the leg A B over that in the leg E F is as $2\ r - 2\ r'$.

But now, supposing all other things to remain the same, conceive the diameter of the tube to vary. A canal $e\ r$, at the same distance from the side, in tubes of different diameters, will be equally attracted by all of them: for that part of the surface of the tube, which attracts the canal, is so small (by the hypothesis) that it may be considered as plane whatever be the tube's diameter.

Therefore, while the diameter of the tube varies, as the number of equal columns in a cylindrical annulus of given breadth, at a given distance from the tube, but within the reach of its action, varies *quam proxime* as the diameter of the tube, while the force urging upwards each separate column is constant, it is easy to see, by collecting as before the sum of the forces acting on diverse annuli, that the excess of the mass in the leg A B over that in E F is as the diameter of the tube.

By combining both parts of the proposition, and supposing the diameter of the tube, and the attractions of its two ends to vary together, the excess of the mass in A B over that in E F will be as $(2\ r - 2\ r') \times$ diameter. Q E D.

Cor. 1st. It is plain from the above demonstration, that the mass of fluid, supported by a capillary tube, depends not in the smallest degree on the figure $c\ a\ d$ of its surface, so that, *if it were possible for us to make this surface take any other conceivable figure, the same mass of fluid would* Corollary 1.

still be supported. The figure of the surface *is a secondary effect*, exactly in the same manner as the catenarian form of a supported chain is a secondary effect; the cause being the pegs and the force of gravity.

When, however, I say, that the figure of the surface is of no consequence, I suppose that surface to be at a sensible distance below the orifice of the tube, otherwise the case will be very different: for,

Corollary 2. Cor. 2d, Suppose, in fig. 5, every thing to remain as before, excepting that the whole tube is made of one kind of matter, (the intensity of attraction for the fluid within it being r) and that the left hand branch terminates at $\gamma\delta$, close to the surface of the fluid. I say that the difference of masses, in the two branches, will, in great measure, depend on the figure of *that part of the surface of the fluid*, at the orifice $\gamma\,\delta$, *which is near to* γ *and* δ: and that we should have the greatest difference of masses if it were possible to make the fluid, at this orifice, take any figure as $\delta\, x\, z\, \gamma$ *, perpendicular at the sides, and having every point of its upper surface distant from γ and δ by a space greater than the radius of the sphere of action of the particles. And the difference of masses in this case, would be to the difference of masses when the surface is horizontal, as $\gamma\,\epsilon\,\delta$, at the orifice of the tube, as 2 to 1.

For, in this latter case, when the surface of the fluid is $\gamma\,\epsilon\,\delta$, it is evident, from the reasoning used in the proposition, that the left hand mass is drawn upwards by a force as r, and the right hand mass by a force as $2\,r$; whence the difference of the masses is as r. But, if the left hand mass could stand at $x\,z$, and the column $\epsilon\,\rho$ become $y\,\rho$, any such column, and consequently the whole left hand mass, would be urged neither upwards nor downwards by the branch $\gamma\,\delta$ of the tube: therefore the difference of masses would be entirely occasioned by the other branch, and would be as $2\,r$.

Now, though we cannot make the fluid stand at $x\,z$, we may give it the convex form $\delta s \gamma$; and if $c d$ was the surface in the branch A B, when that in the other branch was $\delta s \gamma$; let this last become convex, as $\delta s \gamma$, and the first shall be $\omega \pi$; the difference of masses, in the latter case, being to that in the former in a ratio approaching the more nearly to that of 2 to 1, the higher we can make the convex surface $\delta s \gamma$, and the nearer its sides at γ and δ are to perpendicularity with $\delta s \gamma$.

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* Mr. Haüy, explaining Abat's experiments after the ideas of la Place, falls, in consequence, into an errour. See "Traité de Physique," tom. 1, p. 243, Ed. 2. "le petit changement de figure lui donne *plus de force*,"

Here then is the *true* and simple explanation of Abat's experiment, which in no respect depends on the figure of the surface, *in the ſenſe that Mr. la Place means**.

Corollary 3.

Cor. 3rd. Let A B p r, fig. 6, represent a tube of such intensity of attraction, that, if it be immersed in a fluid the horizontal surface of which is f c d, this surface shall undergo no alteration.

Suppose this tube cut off close to the surface; as, by this, the part $o f c$ (a quadrant to the radius of the sphere of attraction) to which half its effect is owing (by the proposition) is taken away, the intensity of attraction of the lower part $f c p$ must be doubled to preserve the equilibrium: and it plainly follows, that the intensity of attraction of the fluid for itself is twice that which the tube before it was cut off had for the fluid.

May I not say, that so *peculiar* a demonstration, of a theorem easily proved in other ways, is of itself sufficient to establish the truth of this theory?

Corollary 4.

Cor. 4th. If, in fig. 4, we suppose the branch E F to be cut off close to the surface (which I suppose horizontal) and then to be of the same intensity of attraction with the fluid it contains, we have the case of a common capillary tube; and the mass raised above the level of the surface of the fluid in the vessel is as $(2r - r')$ × diameter of the tube. From this corollary the explanation of the common phenomena is too simple to make it necessary for me to dwell on it.

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* After the same manner is explained another experiment mentioned by Mr. la Place, 1st supplement, p. 60. Plunge a capillary tube into water, and, having closed the lower orifice with the finger, draw it out of the water. If we now remove the finger, the fluid will fall in the tube, and form a convex drop at the lower orifice. But, when it has ceased to descend, the height of the column always remains greater than the height of the water in the tube, above the level, when it was plunged in the fluid. "*This excess* (says Mr. la Place) *is owing to the action of the drop of water on the column.*"

The true explanation is the same as that I have given above of Abat's experiment.

VI.

An Account of the Effects of Thirty Tons of Quicksilver escaping by the rotting of leathern Bags into the Bilge Water, on board the Triumph Man of War: communicated by Dr. BAIRD, *Physician General to the Navy, to a Friend in London.*

Quicksilver taken on board a ship

in wet leathern bags, which burst.

IN April, 1810, the Triumph man of war took on board thirty tons of quicksilver, contained in leathern bags of 50lbs. each. These bags were picked up on the shore of Cadiz, from the wreck of two Spanish line of battle ships, lost in the storm immediately preceding, at the end of March, the above date. The collected bags were stowed below, in the bread room, after hold, and store rooms: they were saturated with salt water, and, in about a fortnight, all decayed and burst. In the act of collecting and endeavouring to save the quicksilver in casks, much of it found its way to the recesses of the ship, beyond the possibility of recovery. Some portion, however, was secreted by the men, who amused themselves in various ways with it, as cleaning their spoons, &c.

The vapour of the bilge water whitened metals, instead of blackening them,

At this period bilge water had collected in the ship, the stench from which was intolerable; and the carpenter's mate, in the act of sounding the well, was nearly suffocated. The effect of the gas escaping from bilge water is manifested, by its changing every metallic substance in the ship black. But in this instance metals of every kind were coated with quicksilver; and a general affection of the mouth took place among the men and officers, to a severe degree of ptyalism, in upwards of 200 men. The ship was sent to Gibraltar, had all her stores taken out, the hold made clean, and all the quicksilver, that could be reached, removed; but near ten tons are supposed still to remain between the ship's timbers below, which can only be cleared away by docking the ship, and dislodging a plank at the most descending part near the keel. Since the process of cleaning the ship has taken place, and a new atmosphere created, all effects from the quicksilver have ceased.

and salivated great part of the crew.

Dr. Baird having requested an explanation from his friend in London, the following account was transmitted to Plymouth.

To Dr. BAIRD.

From well established principles, as well as analogies, a very reasonable explanation may be given of the effects attributed to thirty tons of quicksilver, exposed on board the Triumph in bilge water, with rotten leathern bags, in a hot climate, the beginning of summer.

This accounted for.

The stinking gas, which was generated, was sulphuretted and perhaps phosphuretted hidrogen gas, mixed with carbonic acid and perhaps other gasses compounded by the putrefaction of animal and vegetable matter. The deadly suffocating effects of which gasses are fully ascertained, unless diluted with a large proportion of fresh air; and the tarnishing of metals, especially of silver, at a great distance, even when mixed with a large proportion of fresh air, is a well known effect of sulphuretted hidrogen.

Gasses naturally arising from bilge water.

These last named effects are attributable to the gasses of putrefaction independently of quicksilver. But when the influence of so large a body of this metal is considered, it will be easy to account for the whitening of metals, and the salivation or sore mouth of many persons in the ship. The quicksilver would rise united or suspended by the above gasses, or be even evaporated by the heat of the ship, in the common fresh air. This metal so suspended or dissolved is very likely to penetrate the human body, and act upon it like the fumigation with quicksilver; but sulphur-

The quicksilver would rise in part from the action of heat.

and likewise from being dissolved by the sulphuretted hidrogen.

etted hidrogen dissolves the metal, and of course would carry it wherever the gas was transmitted.

It will be understood, that the sulphur and phosphorus are furnished probably by the decomposition of sulphuric and phosphoric acids, always present in all kinds of animal matter. The hidrogen gas is furnished chiefly by the decomposition of the water. The carbonic acid is compounded by the union of the charcoal of the animal and vegetable matter with the oxigen (principally) of the water.

Sources of the several gasses,

and of the stench.

The stink is now imputed to the mixture of sulphuretted and phosphuretted gasses with putrifying matter. If the leathern bags of quicksilver had been kept dry, they would not have putrefied, but probably would have retained the metal, and the above effects would not have happened.

VII.

Scheme for preserving the Lives of Persons Shipwrecked.

By G. Cumberland, *Esq.*

SIR,

Hints for preserving persons shipwrecked.

ENCOURAGED by your ready insertion of such papers as I have addressed to you at my leisure moments, that I thought may be of use to society; I take the liberty to propose the publication of some crude reflections on a subject of very great national importance: and although once presented to the Admiralty * without producing even an acknowledgment, I feel, that you will not think such an idea fit to be totally rejected; as, if not immediately put into practice, it may, by being recorded, be the means of ultimately producing, from better heads, some improved provisions, that shall render naval services less dangerous to those, who are the support, the defence, and the bulwark of the nation.

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* My letter was addressed to Lord Melville, then first Lord of the Admiralty.

The

Invention of bending ship timber ruinous to the projector.

The grandson of the man who first invented the bending of ship timber by means of hot sand, in the very cases, which now are filled with boiling water; who ruined himself by expending £16,000 to enrich his country, was rewarded with a delusive patent, and left his children in want; may be allowed to be disinterested in any proposal he makes for the benefit of a navy, that, as individuals, has only been to them productive of disappointment and irretrievable loss.

Natural phenomenon that suggested the idea to the author.

About six years past, a solitary inhabitant of a promontory projecting into the Severn Sea, called Weston Super Mare, I amused myself much among the rocks there, and spent many hours in studying the action and form of water when impelled in the figure of a wave; it being my opinion at that time, as it still is, that the forms water takes from motion are so determined, that even in sculpture they may be represented with correctness; and that nothing would better teach us the art of representing motion by fixed lines, than these images so often repeated with exactness. On these occasions I frequently observed extensive masses of the sea weed called tang on that coast, and which the farmers burn for manure, floating into the hollow coves below me, on the surface of the most tremendous waves; and forming, if I may so express myself, a green carpet, that, undulating on the broken wave, was never submerged, although continually varying its surface; and on which, as on a resting place, birds frequently alighted, or sat to repose themselves, as if it were a verdant down.

Its practical application

On a coast so remarkably dangerous, where no boat could land even in comparatively tranquil weather, these *safe rafts* were very interesting, and led naturally to the thought, whether such a sort of raft might not be constructed of other materials, fit, instead of birds, to carry men. The result of which was, it appeared to me, that if each sailor in a man of war had a cork mattress, and these mattresses were all linked together by cords, such a float, capable of landing safely even on breakers, would be produced.

Cork shavings would make cheap & good mattresses, and effectual for the purpose.

Pleased with the thought I went to Bristol, and consulted a cork cutter there as to the quantity of cork necessary to support a man; and soon found, that a very moderate weight would do, and that cork shavings were then worth only 8*d*. per bushel, and chiefly sold for firing, or to make guards for privateers to fill the nettings.

It therefore struck me, that, as mattresses are necessary in the navy for the hammocks, and nothing dryer than cork or easier to shave into a thin elastic body, it might answer the above end, to fill these mattresses with this substance, in a proportion equal to the support of a single man: and then a mass of them thrown overboard linked together by ties at each corner, where cords might be always attached, would form an extensive raft, capable of sustaining, out of the water, as many men as there were of these mattresses united; and thus conveying them on the tops of the waves, and depositing them safely on shore, or even on the surface of rocks, when the sea retired with the tide.

This plan suggested to the Admiralty.

To contemplate such a thought in imagination is truly delightful; but to believe, as I do, that the thing is practicable with ease, and not communicate it to others, is impossible. I have therefore done all in my power to extend the idea from my own bosom to the mind of the public at large, having first addressed my wishes and plan to that quarter, where the power of putting it extensively into execution alone exists.

As your Journal must ultimately reach all countries, I therefore wish to deposit these reflections in it, in the hope, that they may thus be extended to some practicable benefit, if not to ourselves, to our neighbours, or some distant clime, where the coasts are equally dangerous: for all other rafts, that I have either seen or contemplated, have this great defect, that they come on shore with too much force, and that the blows they receive either disjoint them, or throw off the people; that their wrecks are more dangerous than the rocks they strand on; and that every time they pitch those on them are covered, and some never may be able to retain their hold or rise again.

Defects of all other rafts.

I am, &c.

Bristol, 17*th Aug*. 1810. G. CUMBERLAND.

VIII.

VIII.

Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.*

(Continued from p. 31.*)*

A TABLE *showing the Number of Trees to be cut out in thinning of Woods, and the Number left standing at every Period of* 4 *Years from* 20 *up to* 64 *Years.*

Table 3. For thinning woods.

IN the 24th volume of the Transactions of the Society of Arts, &c., page 75*, Mr. Salmon, in a paper on the Management of Fir Woods, says, " the distance of trees from each other should be one fifth of their height." At this distance, which is probably sufficient for fir trees, the following will be the number on an acre, and the number to be cut out at the ages and heights under-mentioned, and the number of feet they will then contain in the bole, when measured to the top of the leading shoot. These trees are supposed to increase twelve inches in height, and one in circumference annually, and to have been at first planted four feet apart.

TABLE III.

Years old and feet high.	Girt.	Contents			Distance	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents
	inch.	ft.	in.	pts.	feet		feet.		feet.
20	2½	0	10	5	4·	2722	2362	839	727
24	3	1	6	0	4·8	1883	2824	494	741
28	3½	2	4	7	5·6	1389	3308	326	776
32	4	3	6	8	6.4	1063	3779	223	792
36	4½	5	0	9	7·2	840	4252	160	810
40	5	6	11	4	8·	680	4722	118	819
44	5½	9	2	11	8·8	562	5194	90	831
48	6	12	0	0	9·6	472	5664	70	840
52	6½	15	3	0	10·4	402	6130	55	838
56	7	19	0	8	11·2	347	6611	45	857
60	7½	23	5	2	12.	302	7076	37	866
64	8	28	5	4	12·8	265	7537		

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* See Journal, vol. XVII, p. 162.

Table 4. For thinning woods.

And if trees be periodically thinned out to the distance of one fifth of their height, and that they increase fifteen inches in height, and one inch and a half in circumference annually, the number of trees on an acre, and the number to be cut out at different periods, and the number of feet they will respectively contain at those periods, will be as under, viz.

TABLE IV.

Age.	Height.	Girt.	Contents.			Distance.	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
years.	feet.	inch.	ft.	in	pts.	feet.		feet.		feet.
16	20	3	1	3	0	4	2722	3402	980	1225
20	25	3¾	2	5	3	5	1742	4246	532	1296
24	30	4½	4	2	7	6	1210	5100	322	1357
28	35	5¼	6	8	4	7	888	5944	208	1392
32	40	6	10	0	0	8	680	6800	143	1430
36	45	6¾	14	2	10	9	537	7644	102	1452
40	50	7½	19	6	4	10	435	8494	75	1464
44	55	8¼	25	11	10	11	360	9355	58	1507
48	60	9	33	9	0	12	302	10192	45	1518
52	65	9¾	42	10	10	13	257	11026	35	1501
56	70	10½	53	7	0	14	222	11895	29	1553
60	75	11¼	65	10	11	15	193	12740	23	1515
64	80	12	80	0	0	16	170	13600		

Remarks.

It will be observed in all these tables, that when trees have doubled their age, there are only one fourth of the number remaining on an acre, in consequence of their distance being doubled; but as each tree will then have increased its contents eight-fold, therefore the number of feet on an acre must be then doubled. Above, at 64 years of age, there is exactly double the number of feet that there is at 32 years of age.

Table 5. for thinning woods.

And if trees be periodically thinned out to the distance of one fifth of their height, and that they increase eighteen inches in height, and two inches in circumference, annually, the number of trees on an acre, and the number to be cut out at different periods, and the number of feet they will then respectively contain, will be as under, viz.

TABLE V.

Age.	Height.	Girt.	Contents.			Distance.	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
years	feet.	inch.	ft.	in.	pt.	feet.		feet.		feet.
12	18	3	1	1	6	4·	2722	3062	839	943
16	24	4	2	8	0	4·8	1883	5021	673	1794
20	30	5	5	2	6	6·	1210	6302	370	1927
24	36	6	9	0	0	7·2	840	7560	223	2007
28	42	7	14	3	6	8·4	617	8817	145	2072
32	48	8	21	4	0	9·6	472	10069	99	2112
36	54	9	30	4	6	10·8	373	11314	71	2153
40	60	10	41	8	0	12·	302	12583	52	2166
44	66	11	55	5	6	13·2	250	13864	40	2213
48	72	12	72	0	0	14·4	210	15120	32	2304
52	78	13	91	6	6	15·6	178	16294	24	2197
56	84	14	114	4	0	16·8	154	17607	20	2286
60	90	15	140	7	6	18·	134	18843	16	2250
64	96	16	170	8	0	19·2	118	20138		

Table 6. for thinning woods.

But if the trees be first planted four feet apart, and be periodically thinned out to the distance of one fourth of their height, and they increase twelve inches in height, and one in circumference annually, the number of trees on an acre, and the number to be cut out at the ages and heights under-mentioned, and the number of feet they will respectively contain in the bole, when measured to the top of the leading shoot, will be as under, viz.

TABLE VI.

Years old & feet high.	Girt.	Contents.			Distance	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
	inch.	ft.	in.	pt.ˢ	feet.		feet.		feet.
16	2	0	5	4	4	2722	1209	980	435
20	2½	0	10	5	5	1742	1512	532	461
24	3	1	6	0	6	1210	1815	322	483
28	3½	2	4	7	7	888	2115	208	495
32	4	3	6	8	8	680	2417	143	508
36	4½	5	0	9	9	537	2718	102	516
40	5	6	11	4	10	435	3020	75	520
44	5½	9	2	11	11	360	3327	58	536
48	6	12	0	0	12	302	3624	45	540
52	6½	15	3	0	13	257	3919	35	533
56	7	19	0	8	14	222	4230	29	551
60	7½	23	5	2	15	193	4522	23	538
64	8	28	5	4	16	170	4835	20	568
38	8½	34	1	4	17	150	5116	16	545
72	9	40	6	0	18	134	5427	14	567
76	9½	47	7	6	19	120	5715	12	571
80	10	55	6	8	20	108	6000	10	555
84	10½	64	3	8	21	98	6301	8	554
88	11	73	11	4	22	90	6655	8	591
92	11½	84	5	11	23	82	6928	7	591
96	12	96	0	0	24	75	7200	6	576
100	12½	108	6	0	25	69	7486	5	542
104	13	122	0	8	26	64	7811	5	610
108	13½	136	8	3	27	59	8037	4	546
112	14	152	5	4	28	55	8384	4	609
116	14½	169	4	5	29	51	8659	3	508
120	15	187	6	0	30	48	9000	3	562
124	15½	206	10	7	31	45	9309	3	620
128	16	227	6	8	32	42	9557	2	455
133	16½	249	6	8	33	40	9982		

Table 7 for thinning woods.

And if the trees be periodically thinned out to the distance of one fourth of their height, and they increase 15 inches in height, and one inch and a half in circumference annually, the number of trees on an acre, and the number to be cut out at the different periods undermentioned, and the number of feet they will respectively contain at those periods, will be as under, viz.

TABLE VII.

Age.	Height.	Girt.	Contents.			Distance	Numb of trees on an acre.	Content of the whole.	Number to be cut out.	Contents.
Years	feet	inch.	feet.	in.	pts.	feet.		feet.		feet.
12	15	2¼	0	6	3	4·	2722	1417	980	510
16	20	3	1	3	0	5·	1742	2177	627	783
20	25	3¾	2	5	3	6·25	1115	2717	341	831
24	30	4½	4	2	7	7·5	774	3262	206	868
28	35	5¼	6	8	4	8·75	568	3802	133	890
32	40	6	10	0	0	10	435	4350	91	910
36	45	6¾	14	2	10	11·25	344	4897	66	938
40	50	7½	19	6	4	12·5	278	5428	48	937
44	55	8¼	25	11	10	13·75	230	5976	37	962
48	60	9	33	9	0	15·	193	6513	29	978
52	65	9¾	42	10	10	16·25	164	7036	22	943
56	70	10½	53	7	0	17·5	142	7608	19	1018
60	75	11¼	65	10	11	18·75	123	8106	15	988
64	80	12	80	0	0	20·	108	8640		

Table 8 for thinning woods.

And if the trees be planted at 4½ feet apart, and be periodically thinned out to the distance of one fourth of their height, and they increase 18 inches in height and 2 inches in circumference annually, the number of trees on an acre, and the number to be cut out at the different periods undermentioned, and the number of feet they will then respectively contain, will be as under, viz.

TABLE VIII.

Age.	Height.	Girt.	Contents			Distance	Number of trees on an acre.	Contents of the whole.	Number to be cut out	Contents.
Years	feet	inch.	feet.	in.	pts.	feet		feet.		feet.
12	18	3	1	1	6	4·5	2151	2419	941	1058
16	24	4	2	8	0	6·	1210	3226	436	1162
20	30	5	5	2	6	7·5	774	4031	237	1234
24	36	6	9	0	0	9·	537	4833	142	1278
28	42	7	14	3	6	10·5	395	5645	93	1329
32	48	8	21	4	0	12·	302	6442	63	1344
36	54	9	30	4	0	13·5	239	7249	46	1395
40	60	10	41	8	0	15·	193	8041	33	1375
44	66	11	55	5	6	16·5	160	8873	26	1441
48	72	12	72	0	0	18·	134	9648	20	1441
52	78	13	91	6	6	19·5	114	10435	16	1464
56	84	14	114	4	0	21·	98	11204	12	1372
60	90	15	140	7	6	22·5	86	12093	11	1546
64	96	16	170	8	0	24·	75	12800		

Remarks.

It is difficult in thinning plantations, to leave the trees at nearly equal distances. The distances stated in all these tables must be considered the average distances. If, for instance, there be 302 trees on an acre, their average distance will be 12 feet, although few of them may stand at exactly that distance.

Table 9 for thinning woods.

If the trees be first planted 4 feet apart, and be periodically thinned out to the distance of one fourth of their height until they are 28 feet high, and to one third of their height afterward, and they increase 12 inches in height and 1 in circumference annually, the number of trees on an acre, and the number to be cut out at the ages and heights undermentioned, and the number of feet they will then respectively contain in the bole, when measured to the top of the leading shoot, will be as under, viz.

TABLE

TABLE IX.

Years old & feet high.	Girt.	Contents.			Distance.	Number of trees on an acre	Contents of the whole	Number to be cut out.	Contents.
	inches	feet.	in	pts.	feet		feet.		feet.
16	2	0	5	4	4	2722	1269	980	435
20	2½	0	10	5	5	1742	1512	532	461
24	3	1	6	0	6	1210	1815	322	483
28	3½	2	4	7	7	888	2116	453	1078
30	3¾	2	11	4	10	435	1271	133	388
36	4½	5	0	9	12	302	1528	80	404
42	5¼	8	0	5	14	222	1783	52	417
48	6	12	0	0	16	170	2040	36	432
54	6¾	17	1	10	18	134	2289	26	444
60	7½	23	5	2	20	108	2530	18	421
66	8¼	31	2	4	22	90	2807		

Observations on Table IX.

On examining several oak woods, it appeared to me, that the distance of one third of their height was not too much, where the trees were from 30 to 40 feet high and upwards. I have therefore calculated a table according to the distance of one fourth of their height, till they are 28 feet high, and according to the distance of one third of their height afterward. Remarks

The timber to be thinned out before the age of 28 years will be the same as in Table VI; but at 28 years of age there are 583 feet more to be cut out according to this table than at the same age in Table VI; there will however be less to be cut out between the ages of 28 and 60 years of age. But if the trees in this table, in consequence of having more room, were to increase 1¼ inch in circumference annually, instead of 1 inch after they are 28 years of age, the produce of an acre at 60 years of age would equal the produce stated in Table VI at the same age; taking into consideration, that the value of the 583 feet excess cut out at 28 years of age would then be more than quadrupled, if the money were placed out at 5 per cent compound interest. A considerable additional increase in circumference may certainly be expected, in consequence of the trees having almost double the room in which to extend their branches, and for the admission of those powerful agents, sunshire and air.

(To be continued.)

IX.

Observations on Saturn's Ring: by Mr. LAPLACE*.

Two conditions necessary to the permanency of Saturn's ring. 1st.

TWO conditions are necessary, to maintain the ring of Saturn in equilibrio round that planet. One respects the equilibrium of its parts: which requires, that the particles on the surface of the ring should not have a tendency to separate from it; and that, supposing this surface to be fluid, it should preserve itself by the different forces with which it is actuated. Without this the continual effort of its particles would ultimately detach them, and the ring would be destroyed, like all those works of nature, which have not in themselves a cause of stability able to resist the action of the forces that operate against it. In the third book of my *Mécanique céleste* I have proved, that this condition can be fulfilled only by a rapid rotary motion of the ring in its plane, and round its centre, still a little distant from that of Saturn. I have likewise shown, that the section of the ring by a plane perpendicular to its own, and passing through its centre, is an ellipsis elongated toward this point.

2d condition.

Indifferent equilibrium of a hollow sphere

The second condition regards the suspension of the ring round Saturn. A hollow sphere, and generally a hollow ellipsoid, the inner and outer services of which were similar and concentric, would be in equilibrio round Saturn, whatever point of the concavity were occupied by the centre of the planet. But this equilibrium would be *indifferent*; that is, if disturbed, it would have no tendency either to

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[*] Journal de Physique, vol. LXIX, p. 241.

resume its original state, or to depart from it: consequently the slightest cause, such as the action of a satellite, or of a comet, would be sufficient to precipitate the ellipsoid on the planet.

does not hold with regard to a ring.

The indifferent equilibrium, which would take place for a hollow sphere enveloping Saturn, does not exist for a circular zone surrounding the planet. I have shown, in the book above quoted, that, if the two centres of a circular ring and the planet did not coincide, they would repel each other, and the ring would ultimately fall upon Saturn. The same thing would take place, whatever the nature of the ring might be, if it were without a rotary motion. But if we conceive, that it is not similar in all its parts, so that its centre of gravity does not coincide with the centre of its figure; and if we farther suppose, that it has a rapid rotary motion in its own plane; its centre of gravity itself will turn round the centre of Saturn, and gravitate toward this point as a satellite, with this difference, that it can move in the interior of the planet. Thus it will possess a stable motion.

The ring therefore has a rapid rotary motion.

Accordingly the two conditions I have mentioned concur to show, that the ring turns in its plane, on its own axis, and with rapidity. The time of its rotation must be nearly that of the revolution of a satellite moving round Saturn at the same distance with the ring; and this time is about 10 sexagesimal hours and a half. Mr. Herschel has confirmed this result by his observations. But how can we reconcile these observations, and this theory, with the observations of Mr. Schroeter, in which certain points of the ring, more luminous than the rest, have appeared a long time stationary? I conceive it may be done in the following manner.

This apparently inconsistent with the stationary spots observed by Schroeter.

Attempt to account for these.

The ring of Saturn is composed of several concentric rings. Powerful telescopes show two very distinctly, which are confounded together by iradiation in weak telescopes. It is very probable, that each of these rings is itself formed of several, so that the whole may be considered as an assemblage of various concentric rings. Such would be the aggregate of the orbits of the satellites of Jupiter, if each left behind a permanent light in its path. The separate rings, like these orbits, must be variously inclined to the equator of the planet: and then their inclinations and the position of their nodes would change in longer or shorter periods, that would embrace several years. Their centres must equally oscillate round that of Saturn, and these circumstances together muſt at length alter the apparent figure of the rings as a whole. Their rotary motion does not perceptibly change this figure, since it only replaces one luminous part by another in the same plane. It is very probable that the phenomena observed by Mr. Schroeter are owing to variations of this kind. But if a point more or less luminous than the rest adhere to the surface of one of the separate rings, this point must move as rapidly as the ring, and appear to change its position in a few hours. We may presume with much probability, that it was a point of this nature, which Herschel observed.

Appearances of Saturn's ring deserve farther examination.

I would invite those observers, who have powerful telescopes, to examine the appearance of Saturn's ring with this view. The variety of these appearances greatly puzzled geometricians and astronomers, till Huyghens found out their cause. The ring at firſt exhibited itself to Galileo under the form of two small appendages adhering to the body of Saturn: and Descartes, who had an unfortunate propensity for explaining every thing in his Principles of Philosophy, ascribes, in the third part of that work, the stationary state of these supposed satellites to Saturn's always presenting the same face to the centre of his vortices. We now know, that this state is repugnant to the law of universal gravitation; and this reason would be sufficient, to induce us to reject the explanation of Descartes, even if we did not know the cause of these appearances. I do not believe, that the ring is immovable, though this would be less inconsistent with that grand law of nature; and I have no doubt that farther observations; made with the view I have just mentioned, will confine the results of the theory, and the observations of Herschel.

X.

On the Mines of Sardinia: by the Count DE VARGAS, *President of the Italian Academy, &c.**

Primitive mountains of Sardinia.

THE district of Barbagia and the province of Ogliastro are composed of granitic mountains, which extend in the form of amphitheatres from the seashore to the summit of Corruboi. These districts display to the mineralogist a vast and instructive study of primitive mountains.

Secondary mountains.

Other chains are of secondary formation, and their ramifications traverse the island in different directions. At every step they exhibit phenomena, which cannot but throw much light on geology.

Volcanic productions.

Lastly, many volcanic productions are found in the vicinity of Guisos, Santa Catharina de Pitturni in the territory of Cuglieri, and San Lussurgiu.

Ores.

But what particularly deserves attention is the great number of metallic veins, which are seen every where in great profusion. All the historians of Sardinia have spoken of this abundance of metallic ores, They were known from the remotest antiquity, for the remains of the labours of the Carthaginians and Romans in working them are still to be seen.

Gold mines.

Formerly, no doubt, gold mines were worked in the island, since one of the interior provinces still bears the name of the Gold Country; but none are now known there.

Silver mines.

Silver mines abound more or less in almost all the provinces. The mountain of Argentiera de Nurra exhibits another very distinct vein, nearly a mile long. This vein is of gray silver ore: its gangue is barytes. The vicinity of the sea, and abundance of wood in this part, are deserving attention. Horn silver is found in several places, as at Sarabus. Native silver too occurs, as near the bridge of San Nicolas, mixed with vitreous or sulphuretted silver.

Silver mixed with lead.

All the lead mines too contain more or less silver. There are some near the river Maggiore at Sarabus, which yield as much as eight or nine ounces in the hundred weight.

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* Journal de Physique, vol. LXVII, p. 357.

Others contain but one, two, or three ounces. But the most celebrated of all is in the district of Tulana, which is said to yield seventy per cent of pure silver. This belongs to several private persons, who work it in secret.

Copper mines.

Copper mines are tolerably abundant in Sardinia. The copper is generally in the form of pyrites. In the district of Sinia are very beautiful malachites.

Iron.

Sardinia contains a large quantity of mines of excellent iron: but the most considerable is that of Arsana, which contains a magnetic iron of superior quality. There is another mine of magnetic iron in a mountain of porphyry at Trulada.

Lead.

Lead mines abound in Sardinia, and all contain some portion of silver. The most considerable is that of Monteponi near Iglesias. It yields 60 or 64 per cent of pure metal. The lead mines of Sarabus are not less interesting.

Zinc.

Blende or sulphuret of zinc, occurs mixed with galena.

Native quicksilver.

On repairing the buildings of a convent at Oristono, native mercury was found in a bed of clay. Some persons say too, that native mercury was found on repairing the public prisons. Chaptal speaks of some having been found in a bed of clay on digging for the foundation of some buildings at Montpellier.

Antimony.

There are a number of mines of antimony at Balland, and at Escala Plana.

Manganese.

A mine of manganese has been discovered at San Pietro.

Coal.

Coal has been found at Tanara near Forni, and at Corruboi.

XI.

Analysis of various Minerals, by Mr. KLAPROTH *.

Black crystallized augite of Frascati.

Pyroxene

ONE of the principal varieties of augite is that found in fine black crystals in fissures in the Latian mountains, near Rome, particularly near Frascati, and formerly called black volcanic schoerl.

Described.

Its figure is commonly a hexaedral prism, bevelled at the extremities, the two faces of the bevel answering to the two lateral edges of the prism †. Mr. Haüy has described this species under the name of *pyroxene*, the chief varieties of which are the *bisunitaire* and the *triunitaire*.

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* Abridged from the Annales de Chimie, vol. LXVII, p. 225, &c. Trans. from Gehlen's Journal.

The surface of these crystals is smooth, sometimes shining, at other times only partly so. Interiorly they have a very glassy lustre.

They are hard, easily broken, and their fracture perfectly conchoidal. When rubbed to powder their colour is a greenish gray.

Their specific gravity is 3·4.

Treated with the blowpipe.

Before the blowpipe, on charcoal kept at a red heat, the angles and edges ultimately become rounded.

Analysis.

A *a*. A hundred grains, reduced to an impalpable powder, were heated red hot with twice their weight of caustic potash. The matter did not enter into fusion. It was of a brown colour, and gave a slight green tinge to the water, with which it was diluted. On supersaturating the liquor with muriatic acid, a complete solution was obtained. On evaporating to dryness, and redissolving in water, the silex was separated. After being heated red hot, it weighed 48 grains.

Silex.

b. The solution was precipitated by ammonia; and the brown precipitate, while still wet, was boiled in a caustic lixivium. The alkaline liquor, mixed with muriate of ammonia, let fall alumine, the weight of which, when purified, was 5 grains.

Alumine.

Oxide of iron.

c. The brown residiuum was dissolved in nitric acid, the solution diluted with a great deal of water, and carbonate of soda added. The oxide of iron precipitated, and heated red hot, weighed 12 grains.

Oxide of manganese.

d. The supernatant liquor was decomposed at a boiling heat by carbonate of soda. The precipitate obtained, and heated red hot, weighed 10½ grains. It had assumed a reddish colour. Being dissolved in nitric acid, it left behind oxide of manganese, weighing one grain after calcination.

e. As the nitric solution appeared to contain magnesia and lime, oxalate of potash was poured in, till no farther precipitate ensued. The oxalate of lime, collected and heated

Lime.

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† The places of these two edges are occupied by trapezoidal facets in the triunitary variety, the prism of which has eight sides instead of six.

Magnesia.

red hot, yielded 4½ grains of lime. The remaining liquor, being decomposed by carbonate of soda, yielded 5 grains of calcined magnesia.

f. The muriatic solution, decomposed by ammonia in experiment *b* was precipitated boiling by carbonate of soda. The precipitate washed and dried weighed 44½ grains. This was neutralized by sulphuric acid, and evaporated to dryness. The hardened mass was triturated and lixiviated gradually with a little water. The solution being evaporated, left sulphate of magnesia, which was decomposed by carbonate of soda, and 9 grains of carbonate of magnesia were obtained. These, being deducted from the 44½ of the first precipitate, left for the carbonate of lime 35½ grains, which amount to 19½ of lime. The 9 grains of carbonate of magnesia being heated red hot, 3¾ grains of magnesia were obtained.

More lime and magnesia.

B. Eighty grains of this stone reduced to an impalpable powder were heated red hot with an ounce of nitrate of barytes. The calcined matter was triturated with boiling water, dissolved in muriatic acid, and then precipitated by carbonate of ammonia. The filtered liquor was evaporated to dryness, and the salt volatilized in a platina crucible. An earthy salt remained, which was redissolved in water, and decomposed by carbonate of ammonia. The filtered liquor was evaporated anew, and the salt volatilized by heat. A slight trace of muriatic neutral salt remained, which was found to be muriate of potash, by its forming a few grains of a triple salt with a solution of platina.

Potash.

A hundred parts of augite of Frascati therefore contain

Component parts.

Silex	A *a*		48
Lime	*e*	4·5	24
	f	19·5	
Magnesia	*e*	5	8·75
	f	3·75	
Alumine	*b*		5
Oxide of iron	*c*		12
Oxide of Manganese	*d*		1
Potash	B		*a* trace
			98·75

This

Pyroxene from Etna analysed by Vauquelin.

This analysis nearly agrees, both in the nature of the component parts, and in their proportions, with that which Mr. Vauquelin has given of the crystallized black augite of Etna; in which he found

Silex	52
Lime	13·20
Alumine	3·33
Magnesia	10
Oxide of iron	14.66
——manganese	2.
	95·19

Hence we may consider these two stones as one variety, though the quantities of lime and alumine are less in that of Etna, and those of the other component parts on the contrary greater.

Analysis of Melanite.

Black garnet.

Another stone is met with at Frascati, and at Albano, near Rome, which is found in single detached crystals, and has been called black garnet.

Described.

Its form is that of the emarginated garnet of Haüy. By trituration it yields a brownish gray or blackish powder. Its specific gravity is 3·7. When heated red hot in a crucible it undergoes no observable change; but before the blowpipe it rounds gradually into a globule.

Action of heat on it.

Analysis.

It is unnecessary to give the analysis in detail, as it was conducted like the preceding, except that the oxide of iron was precipitated by succinate of ammonia, and the following method was pursued for detecting a fixed alkali.

Examination for alkali.

Sixty grains were decomposed by muriatic acid; but this was not effected completely till after several repeated digestions. After separating the silex, the solution was decomposed by carbonate of ammonia. The ammoniacal liquor was evaporated to dryness, the residuum redissolved in water mixed with carbonate of ammonia, filtered, and evaporated again. The salt, volatilized in a platina crucible, did not give the least trace of a fixed alkaline salt.

The component parts of the melanite were found to be

Component parts.

Silex	35·5
Lime	32·5
Alumine	6
Oxide of iron	24·25
——— manganese	0·4
	98·65

This analysis agrees very closely with that of Vauquelin. The melanite differs greatly therefore from augite, and particularly in containing no magnesia.

Bohemian garnet.

In a note Mr. Klaproth observes, that his analysis of the Bohemian garnet, now called *pirop*, has been given in several French works as his analysis of the melanite; and he has found, since the publication of the 2d volume of his Essays, that about 2 per cent of chromic acid should be added to the component parts of the Bohemian garnet as there given.

Analysis of the staurolite (staurotide of Haüy).

Cruciform schoerl.

Mr. Klaproth has analysed two varieties, one red the other black, both from St. Gothard.

Red.

The proximity of the red staurolite to the cyanite is very remarkable. These two substances are frequently crystallized together, so as apparently to form but one stone. When this is the case, the staurolite becomes a little translucid at the end of the prism.

Brown.

The brown staurolite of Quimper, in the department of Morbihan, as well as that of Finistere, in France, serves as a connecting link between the black and red varieties. In this country conjoined crystals are much more common than single ones; and they commonly cross each other at right or at oblique angles (the *staurotide rectangulaire* and *obliquangle* of Haüy). Frequently the crystals joined together are of the same size; but often one is smaller than the other, and seems implanted in the larger.

Black.

The specific gravity of the black staurolite was 3·51. It experienced no change of colour, weight, or figure, by calcination. Its component parts were

Component parts of the black,

Silex	37·5
Alumine	41
Oxide of iron	18·25
Magnesia	0·5
Oxide of Manganese	0·5
	97·75

The specific gravity of the red staurolite was 3·765. Its component parts

of the red.

Silex	27
Alumine	52·25
Oxide of iron	18·5
——— manganese	0·25
	98

Analysis of hypersten, called Labrador hornblende.

Labrador hornblende.

Mr. Haüy was the first who distinguished this stone from hornblende. He had classed it with the metalloid diallage, or our bronzite; but he has lately shown, that it differs both from hornblende, and from the diallage, or smaragdite. He designates it under the name of metalloid reddish brown lamellar hypersten.

Spec grav. Action of heat.

Its specific gravity I find to be 3·39. Before the blowpipe it is infusible, but its semimetallic lustre is turned blackish. If exposed to a red heat after trituration, the powder, which was of a deep ashen gray, acquires a brown red colour, and loses one per cent of its weight.

Component parts.

The results of its analysis were

Silex	54·25
Magnesia	14
Alumine	2·25
Lime	1·5
Oxide of iron	24·5
Water	1
Oxide of manganese, a trace	
	97·5

Analysis

Analysis of the stangenstein of Altenburg (pycnite of Haüy).

White schoerl of Altenburg.

As this stone has been termed shoerllike beryl, Mr. Klaproth examined it for glucine, after this earth had been discovered in the beryl and emerald by Vauquelin; but he could not find the least trace of it. He had formerly observed the great difference between it and beryl, when they were exposed to the heat of a porcelain oven; as this stone lost twenty-five per cent, and the beryl but one. This led him to conclude, in 1800, that it contained the same volatile matter as the topaz. Mr. Bucholz, apparently without knowing this had been mentioned by Mr. Klaproth, found it to be the fact; which was afterward confirmed by Vauquelin. Mr. Klaproth, having since analysed it with great care, obtained the following results.

Component parts.

Silex	43
Alumine	49·5
Oxide of iron	1
Fluoric acid	4
Water	1
Loss	1·5
	100

Allied to the topaz.

Vauquelin.

This analysis shows, that it is nearly allied to the topaz. The 3·3 per cent of lime found by Mr. Vauquelin are supposed by Mr. Vauquelin himself, to have been owing to the impurity of his specimen.

Analysis of the reddish tourmalin of Moravia.

Reddish Moravian tourmalin.

This tourmalin is found in the mountain Hradisko, near Roczna, imbedded in a compact whitish gray quartz, or in lepidolite. It is in the form of prisms, or needles, of a peach-blossom colour, which verges in several parts to greenish, yellow, and gray white. As it is met with immediately under lepidolite, it has been taken for lepidolite crystallized; and it is under this name that Estren has given a very minute description of it, to which I refer the reader. By some preliminary trials it was soon found, that this stone was not a lepidolite; and it was then classed with the schoerlaceous beryl, or stangenstein. Mr. Haüy placed it with the red schoerl of Siberia, or siberite, with much more reason, as will appear by the following analysis; and he even classed it among the tourmalins, because its crystals have the property of attracting and repelling light substances, when they are heated. As it is not fusible like the tourmalin however, he distinguished it by the epithet apyrous.

Spec. grav.

The specific gravity of the crystals detached from the quartz varies, according as they are more or less old, from 2·96 to 3·02.

Its component parts are

Component parts.

Silex	43·5
Alumine	42·25
Oxide of Manganese	1·5
Lime	0·1
Soda	9
Water	1·25
	97·6
Loss	2·4
	100

Rubellite tourmaline.

The component parts of this stone therefore, and their proportions, completely justify Mr. Haüy, even in a chemical view, for classing it with the siberite, or apyrous tourmalin; since, from a recent analysis of the latter by Mr. Vauquelin, it is composed of

Component parts.

Silex	42
Alumine	40
Oxide of manganese, a little ferruginous,	7
Soda	10
Loss	1
	100.

(To be continued.)

XII.

Method of curing the Footrot in sheep. By Mr. RICHARD PARKINSON, *of Walworth*[*].

SIR,

THE enclosed is the recipe for the cure of the footrot in sheep, certified by the person who was my shepherd at the time I put the method into practice.

I am, Sir, your very obedient servant,
18, *Harford Place, Walworth*, R. PARKINSON.
April 6, 1807.

To Cure the Foot Rot in Sheep in the best and most effectual Manner.

Cure for the footrot in sheep.

In sheep thus affected, pare their hoofs, leaving no hollow to hold dirt; if there be matter formed, be particularly careful to let it out; after which, take some stale urine and wash their feet clean from dirt, and wipe them with a sponge; then put the sheep into a house or shed, the floor of which has been previously spread about two inches thick with quick lime, reduced to powder by a small quantity of water. The fresher the lime is from the kiln the better. Let the sheep stand upon it for six or seven hours, and the cure will be effected.

Testimony of its efficacy.

A certificate, dated March 27, 1807, from Joseph Dunnington, stated his being shepherd to Mr. Parkinson, at Slane in Ireland, in the year 1803, and that he then witnessed the efficacy of the above remedy on a large flock of sheep.

Farther certificates from the Earl of Conyngham, from Mr. Stephen Parkinson, and from Joseph Preston, shepherd to Mr. John Parkinson of Bolingbroke, confirmed the above statement.

XIII.

On the Use of the Italian Poplar for supporting the Vine and Hop[†].

Poplar used to support the vine,

IT is well known, that in Italy the poplar is employed as a support to the vine. When thus used, it is frequently lopped, that its branches may not spread so as to be injurious.

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[*] Trans. of the Soc. of Arts, vol. XXVI, p. 126. The silver medal of the Society was voted to Mr. Parkinson.

[†] Sonnini's Bibliotheque Physico-écon. Nov. 1808, p. 311.

and the hop.

Mr. Hubert, counsellor of the bailiwick of Iphofen, in Franconia, says, in a paper on the cultivation and use of the poplar, that it may be planted to support the hop, and would be an advantageous substitute for the poles usually employed, which occasion a considerable consumption of wood.

Carolina poplar recommended:

The poplar, particularly the Carolina, *populus angulata*, grows in the poorest soil, its leaves are good food for cattle, and its wood is employed for various purposes. Much would be saved therefore by employing it in our hop-grounds. We may presume it would not deprive the hop of its nutriment; and its leaves, after having sheltered the hop from injurious winds, would serve as manure when they fell.

but perhaps the Italian preferable.

Every species of poplar does not appear to be equally well adapted to the support of the hop, The Italian poplar, *populus fastigiata*, perhaps deserves a preference.

Its advantages

Beside its growth being very rapid, as it attains the height of 60 or 70 feet in 20 years, its branches do not spread so much as those of other species. If barked a twelvemonth before it is felled, or indeed when cut down if it be at the time the sap is rising, its wood acquires great hardness, and it is not liable to be injured by the worm. As fuel indeed its quality is but indifferent, as it does not afford much heat.

Wood

XIV.

Analysis of the Root of Valerian: *by Mr.* TROMMSDORFF[*].

Valerian root loses 0·75 by drying.

THE root of valerian, *valeriana officinalis L.*, loses 0·75 by drying. Twelve pounds of the dried, or 48 of the fresh root, distilled with water, yield 2 ounces of volatile oil. This oil is very fluid, and of a greenish cast. Its smell is strong, penetrating, and more camphory than that of the root. Its spec. grav., at 20° R. [77° F.], is 0·934. Its taste is aromatic, and camphory, but not burning. The action of light

Essential oil.

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[*] Annales de Chimie, vol. LXX, p. 95.

METEOROLOGICAL JOURNAL,

For SEPTEMBER, 1810,

Kept by **ROBERT BANCKS,** Mathematical Instrument Maker,
in the STRAND, LONDON.

AUG. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night	9 A. M.	Day.	Night.
27	65°	62°	70°	54°	30·09	Fair	Fair
28	60·5	65	6[illegible]	54	30·16	Ditto	Ditto
29	62	67	68·5	60	30·15	Ditto	Cloudy
30	65·5	67	72	60	30·03	Ditto	Ditto*
31	65·5	71	75	63	29·92	Ditto	Ditto †
SEPT.							
1	68	72	77	62·5	29·88	Ditto	Fair ‡
2	70·5	73	79·5	63·5	29·94	Ditto	Ditto
3	67	62	62·5	55	29·85	Rain	Rain
4	61	61·5	62	50	29·70	Fair	Fair
5	55	60·5	62	53·5	30·02	Ditto	Ditto
6	60	56	66	48·5	30·00	Ditto	Ditto
7	55·5	58·5	64·5	48	30·35	Ditto	Ditto
8	54·5	60	64	48	30·18	Ditto	Ditto
9	53·5	61·5	68	52·5	30·10	Ditto	Ditto
10	57	55	66	48	29·96	Rain	Ditto
11	53·5	54·5	65	48	29·90	Cloudy	Rain
12	53	52·5	57	44·5	29·69	Rain	Fair
13	50	57	59	54	30·07	Ditto	Ditto
14	60·5	61	66	48	30·16	Fair	Ditto
15	53	56·5	59·5	49·5	30·37	Ditto	Cloudy
16	57	59	62	56	30·27	Rain	Ditto
17	61	62	65	53·5	30·11	Fair	Ditto
18	53	61	63	58	30·05	Ditto	Ditto
19	60	62	64	54	30·10	Ditto	Ditto
20	58	60	63·5	56	30·12	Ditto	Ditto
21	61·5	62	65	55	30·09	Ditto	Ditto
22	61	61	67	51·5	30·03	Ditto	Ditto §
23	56·5	57	59	51	29·92	Rain	Fair
24	56	59	61·5	55	30·10	Fair	Cloudy
25	59·5	60	65	53·5	30·15	Ditto	Fair
26	60·5	60	66	52·5	30·11	Ditto	Ditto

* Rain at 11. Thunder and lightning in the Night.
† Thunder, lightning, and heavy rain.
‡ Cloudy in S W. § Lightning.
Rain, 1·89 of an inch, since last Journal.

A

JOURNAL

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AND

THE ARTS.

NOVEMBER, 1810.

ARTICLE I.

On the Electric Column. By J. A. De Luc, *Esq. F. R. S.*

Part II.

On the Electric Column, as Aerial Electroscope.

The sticking of the gold leaves disadvantageous.

Have said in the preceding paper, that, the *sticking* of the *gold leaves* to the side of the electroscope being an obstacle to regular observations of their *strikings*, which however I considered as the most proper manner of observing the variations of this instrument, I had intended to increase its power, so far as to produce the motion of a small metallic *ball*, in hopes that the latter would not *stick*.

Attempt to move a metallic ball.

I began this attempt by uniting together in one the three *columns* of 200 groups mentioned in the *first Part*, which I had before used by only connecting them with one another; and I thus formed the *column* of 600 groups making a part of the instrument represented by the *figure* annexed to the first part: see Pl. III. With this *column* I made the first trial of procuring the motion of such a small *pendulum* in the following manner. I connected with each extremity of the *column* a wire terminated by a small brass *ball*; and each wire being movable, I could bring the two *balls* in front of the instrument, within the distance of each other that might be found convenient; and I suspended, by a silk thread, a small gold *bead*, which I could easily bring between the *balls*, either at the middle distance, or nearer one or the other.

Unsuccessful trials with an insulated ball.

The apparatus being thus prepared, I tried for a long time to make it answer my purpose, but without success. When the two *balls* were near each other, the *bead* struck them alternately with such a rapidity, that it was impossible to count the number of oscillations in a determined time; a necessary condition for my purpose. I increased the distance of the *balls*, but I was disappointed in a different manner: when the *bead* was equally attracted on both sides, it sometimes remained motionless for hours in the middle of the interval; and when the attraction became stronger on one side, the *bead*, drawn that way, struck one of the *balls*, some oscillations then began, still too rapid and very irregular, but at the end of a little time, they ceased again, the bead remaining motionless at the middle point. I tried various distances of the *balls*, and also different degrees of approximation of the *bead* to either of them: sometimes there was an appearance of success, but at last the bead was again at rest in the middle space, or it *stuck* to one of the *balls*. This want of success persuaded me, that a *neutral* pendulum, such as was the *bead* suspended by a *silk* thread, could never answer the purpose of the regular *strikings*, which were necessary; that the *bead* was to be connected, by a *metallic* wire, with one of the extremities of the *column*, near a *ball* united with the latter, and to *strike* against another *ball* connected, either with the other extremity of the *column*, or with the ground; the latter of which modes I first adopted.

Mr. Forster had succeeded with a longer column, to produce a continual chime.

But before I proceed, I must mention, that while I was employed in these trials at Windsor, Mr. B. M. Forster had succeeded at Walthamstow, near London, with a power of 1500 groups of the same diameter as mine, divided into three chaplets, as described in my first paper presented to the Royal Society, to set in motion a brass ball, suspended by a silk thread between two small insulated bells, connected with the two extremities of this long *column*. Mr. Forster has described this new kind of *chime* in a letter to Mr. Tilloch, published in the *Phil. Magaz.* of the latter, mentioning this singular circumstance, that the ringing began on the jubilee day. Since that time I have had the pleasure of being personally acquainted with Mr. Forster, and he has lately informed me, that, having mounted again this chime on the 25th of March, it has not ceased to ring ever since. This is a curious application of the property of the *column*, but, as I have explained, it cannot answer the purpose of regular observations.

Addition to the apparatus.

For the execution of the above mentioned plan, I first made the following addition to my apparatus. At the top of one of the pillars of the *column*, on its side A (the positive extremity, I fixed a brass piece 13, held there by a ferrule, and projecting forwards about 1½ inch: on this projection is fixed, by a screw, another brass piece, having on one side a vertical groove 14, in which is held, by a pin, a brass rod, at the lower part of which is a large brass *ball* 15, which can be moved backwards and forwards, in order to bring it to the convenient point, where it remains steady, by the friction of the top of the rod in the groove. From this top projects a brass loop 16, to which is suspended a gold *bead* 17, by the thinnest silver wire, such as is used for cross wires in telescopes; and by moving properly the *ball* 15, the *bead* is made to hang close to it, without leaning against it: this is done while both the *bead* and the *ball* are *neutral*, by handling the latter for this adjustment.

My purpose having only been at first, that the *ball* 18 should be in communication with the *ground*, I produced this communication in a simpler manner than is represented in the *figure*; having changed it since for a purpose that I shall explain; but the difference is here of no consequence: it was then only held at the top of a brass stem, fixed to the end of a thick slip of lead 19, 19; movable backward and forward between pins 20, 20, 20, in order that the distance of this *ball* from the *ball* 15 might be changed, as should be found proper for the *strikings* of the *bead*.

This apparatus was finished in the beginning of last spring; but it was too late for the most important observations; especially as the apparatus itself was far from being settled. Before that season, the effect of the *column* had been so great, that the *gold leaves* of the electroscopes, either on one side or the other, *struck* sometimes in the afternoon 60 times in a minute, even in glass tubes of 1½ inch diameter; but now they seldom *struck* more than once in a minute, *sticking* always as usual (which was the reason why I had given up the *gold leaves*): however the effect was still sufficient to try the new apparatus.

The ball would still stick.

This again gave me much trouble: for though at first it appeared to answer my purpose; as the gold *bead*, receding from the *ball* 15, struck the ball 18, fell and returned again, with the usual changes in the *frequency* which were to be the object of observation; the *bead* at last *stuck* to the *ball* 18. I tried whether, by increasing the distance of the latter, the *bead*, thus drawn farther out of the vertical line, would have more power to resist the cause of its *sticking*; which it is difficult to understand, as it is not the case when set in the same motion by a mechanical impulse. The *strikings* were less frequent; not so however as to prevent the observation, if the *sticking* had been prevented; but it again took place, and even more easily, as the *bead* arrived more slowly in contact with the *ball*. I did not succeed better by increasing the power (in a manner which I shall explain hereafter), though I could produce the *strikings* at a greater distance; so that, after much labour, I had some time despaired of success, when another idea occurred to me, which however did not succeed at first.

Attempt to produce the striking by the wire, not by the bead.

The general idea was, to produce the *strikings*, not by the *bead* itself, but by its silver *wire*, in a part at some height above the latter, by the *wire* meeting there the edge of a horizontal brass lamina in communication with the ground; in order that the *wire* being bent at that point, by the *bead* moving still farther, the latter should have a greater angular motion from that point, by a shorter *radius*, and have thus a greater tendency to fall back after being discharged; suppressing at the same time the *bal.* 18, to which it *stuck*: but by this suppression, the motion was so much diminished, that I was obliged to use again the *ball*; and then the *wire* itself *stuck* to the edge of the lamina. Following however farther the idea of discharging the *bead* by its *wire* meeting with the smallest *conducting* mass possible, I thought of substituting for the brass *lamina* a single silver *wire* like that of the *bead*; and at last I thus succeeded. This is the part of the *figure* which I am going to describe.

Farther addition to the apparatus.

The last alteration I found necessary to make in the apparatus, which is represented in the *figure*, not being made at that time, it must be supposed for the present, that the piece 28 is represented by the lead slip 19, 19. The piece 23 is a brass spring of about half an inch in breadth at the base 24, where passing under the bent part of an upright brass piece 25, it is thus fixed with the latter, by screws, on the base. The breadth of the spring 23 diminishes toward its end, where it is terminated by a brass wire *bow* 22; in this is stretched the thin silver *wire* 21, against which that of the *bead* comes to *strike*. The upright brass piece 25 has at the top a screw 26, pressing against the spring, and serving to produce small motions, backwards or forwards, of the horizontal wire 21, previously brought nearly to the proper distance, by moving the lead base 19, 19. The moment of the meeting of the two *silver wires* is to be an instant before the *bead* strikes the ball 18: then, by a jerk produced at the meeting of the *wires*, the *sticking* of the *bead* to the *ball* is effectually prevented.

The strikings affected by the least shake.

This was only finished in the beginning of last April; the *strikings* of the *bead* were then regular and uninterrupted, while there was no shake of the apparatus itself; but being on a table, I soon found, that by walking in the room, and also by the agitation of the air in opening and shutting the door, the motions of the *bead* were disturbed. This determined me to fix, against the side of the room which had a proper light, a glazed box, in which I placed the apparatus; and I fixed under it, at its level, a little table, in order to place there additional *columns*, which became necessary to increase the power of the instrument.

Journal of the performance of this instrument.

This apparatus being at last ready for regular observations, I began the meteorological journal which I had in view, including, with the *number* of the *strikings* of the *bead* in a certain time and the observation of the *barometer*, the degrees of the *thermometer* and of my *hygrometer* in the room, the only place of which it could be supposed that the changes of the *temperature* and of the degree of *moisture* might affect the state of the column: I shall copy here the journal of these observations during the few days in which the fundamental *column* of 600 groups still acted alone; after which time I wes obliged to increase the number of the *groups*.

April			*Barom.*	*Thermom.*	*Hygrom.*	*Numb of strik. in 10 min.*
8.	8	A.M.	29·7	57	41	11
	11		id.	60	id.	10
	1	P.M.	id.	60½	id.	12
	3		id.	63	id.	20
	5		id.	62½	id.	10
	7		id.	61	id.	8
	11		id.	61	id.	7
9.	8	A.M.	id.	58	42	11
	11		id.	60½	id.	9
	11	P.M.	id.	63	41	3
10.	11	A.M.	29·8	61	id.	6
	2	P.M.	id.	60	id.	5
	11		29·9	58	id.	2
11.	7	A.M.	id.	50	id.	3
	4	P.M.	30	56	id.	2
12.			30·1	No striking the whole day.		

Cessation of the striking.

This cessation of striking having lasted two days more, I judged, that we were entering into the season, when, in the two years before, the *strikings* of the *gold leaves* themselves had also ceased; so that in order to carry on the observations as long as possible towards the summer, it was necessary to increase the number of the *groups*. This I undertook without changing the situation of the fundamental instrument, which, on account of the necessary steadiness, and of being sheltered from currents of air, was to remain in the glazed box fixed against the side of the room. I made use therefore of the little table above-mentioned, for placing on it additional columns, which I made upright like the common *pile*, as more easily managed; and knowing that with the same number of groups, the *strikings* would be accelerated by larger plates, but that it was a long and tedious operation to cut them round, I determined to make a *column* with square plates. For this purpose I bespoke some sheets of laminated *zinc* about the thickness of a card; but those which I received were so much puckered, that I despaired of their being fit for my purpose; however I obtained flat plates of them by a method which it may be useful to explain.

Sheets of zinc not smooth,

I procured a good pair of hand-shears, and with these I first cut the puckered sheets into slips of 1¼ inch breadth, as nearly as they could be traced upon such an uneven surface; and placing many of these slips upon one another between two pieces of hard wood, I pressed them with force in a vice, leaving them there for half an hour: they came out very flat, only not very straight, but this could be mended. *Zinc*, in this malleable state, having nearly the softness of *lead*, stretches laterally by that great pressure, and thus the puckerings are effaced. Making then straight, with a file, one side of the slips, I marked on this edge, with a divider, points at 1¼ inch distance; and by these points I traced with a square the plates to be cut with the shears: these pieces were distorted by the cutting; but placing them also over one another, by scores, between two thick plates of brass, and pressing them strongly in the vice, they became flat; and I had only to round a little the angles with a file, placing them again in the vice without the brass plate.

but may be made so by strong pressure.

In this manner, I made 300 *zinc* plates 1¼ inch square, and having cut an equal number of pieces of *Dutch-gilt paper* of the same size, I mounted this upright *column* between 4 glass rods covered with sealing-wax, fixed in a wooden base. This *column*, loose between the rods, is supported at the bottom on 4 insulating pillars 1½ inch high, on which is first laid a brass plate with a projecting part of about 2 inches, at the extremity of which is a large hole for receiving the end of proper conductors; and for the same

An upright column added.

purpose, the column is terminated at the top by a similar plate, on which presses a screw in the common manner used for the *pile.* The top of this *column*, which is its *positive* extremity, was to be connected with the *negative* extremity of the horizontal *column*, and this required, that the box containing the latter should be opened in front: I therefore placed only a pane of glass on the side where the *bead* hangs, in order to guard it against the motions of the air.

The first use which I made of this additional *column* with *larger plates* was for the following experiments.

Exp. 16. *Exper.* 16. Placing the *negative* (or lower) extremity of the *column* composed of the large square plates, in communication with the ground; I connected its *positive* extremity with a *gold leaf* electroscope, and after having observed the *maximum*, soon produced, of its *divergence*, I substituted for this *column* 300 of the small groups of the horizontal *column*, by placing the communication with the ground at its middle point: this produced the same divergence as the former, but it required more time.

I could not compare directly the effects of the two *columns* with respect to the *frequency* of *strikings* of the *bead*, because at that time 300 groups of any size were no longer sufficient for producing them; but I compared the effects of the two *columns*, for this purpose, in the following manner.

Exp. 17. *Exper.* 17. I first repeated the observation of the *strikings* with the horizontal column of 600 groups, its *negative* extremity being in communication with the ground: there were 3 *strikings* in 5 minutes. I then took off the communication of this *column* with the ground, and connecting its middle point with the *positive* extremity of the column of 300 groups of the square plates, I placed the *negative* extremity of the latter in communication with the ground. This was again 600 groups, but 300 of them were of *larger* plates, and there were then 7 *strikings* in the same time; and thus was confirmed what I had judged of the effect of larger plates for increasing the *frequency* of the *strikings*.

Exp. 18. *Exper.* 18. I now connected the new *column* of 300 larger plates with the 600 groups of the horizontal column, leaving the communication with the ground at the *negative* extremity of the former. This was in a more favourable moment; for the addition of 300 of the small groups ought to have produced with the whole only 8¼ *strikings* in 5 minutes, and there were 10.

Diminution of the strikings

From this increase of power, I expected a greater duration of observations in this season; but I soon saw a diminution in the frequency of the *strikings*, which disappointed me. I lost again much time in changing the arrangement of the apparatus, in order to take in also the two *columns* formed of *tinned-iron* plates, mentioned in my first paper: there were 700 of these groups, which produced on the *gold-leaves*, nearly the same *divergence* as the *column* of 300 zinc plates 1¼ inch square: This being the only change that I could undertake for the season, I began another course of observations; and it will be seen in the following journal, how rapidly the effect went on diminishing.

went on rapidly.

May			*Barom.*	*Thermom.*	*Hygrom.*	*Numb of strik. in 5 min.*
10.	8	A.M.	30·15	63	40	14
	12		id.	67	id.	19
	2	P.M.	30·17	67	id.	17
	4		30·20	66	id.	16
	8		30·25	65	id.	12
	10		id.	65	id.	10
11.	7	A.M.	id.	59	41	7
	11		id.	64	39½	8
	2	P.M.	id.	67	id.	11
	4		id.	67	id.	9
	10		id.	64	39	4
12.	7	A.M.	30·19	57	39½	4
	11		30·18	60	39	4
	3	P.M.	30·15	65½	38¼	6
	10		30·10	65½	39	4½
13.	9	A.M.	30·05	63	40	7½
	1	P.M.	30·02	65	39½	7
	10		30	64	39¼	2
14.	9	A.M.	29·85	62	39½	3
	12		29·80	65	id.	7
	3	P.M.	id.	67	id.	8
	11		29.75	66	39	*no striking.*

May			Barom.	Thermom.	Hygrom.	Numb. of str. in 5 min.
15.	7	A. M.	29·65	61	40	
No *striking* the whole day.						
16.	7	A. M.	29·6	64	40½	½ or 1 in 10 mi
	11		29·65	63	39½	5
No *striking* in the evening.						
17.	7	A. M.	29·83	59	40	1
	10		id.	63	39¼	3¼
	11	P. M.	29·65	66	id.	5
18.	7	A. M.	29·75	59	40	3¼
	12		29·80	63	39¼	3¼
	11	P. M.	29·95	61½	39	2
19.	7	A. M.	30·1	56	39½	*no striking.*
	4	P. M.	30·2	65	id.	4
No *striking* in the evening.						
20.	8	A. M.	30·18	57	39½	2½
	4	P. M.	30·08	67½	39	4
	10		30	67	38¾	2
21.	7	A. M.	29·9	60	40	½
	2	P. M.	29·83	66	39¾	5½
	11		29·9	65	40	3
22.	7	A. M.	30·1	60	40½	2½
	5	P. M.	30·15	69	40	5½
	10		30·2	64	40½	4½
23.	8	A. M.	30·3	60	40½	3½
	12		id.	63	39½	4
No *striking* for a long time.						
	7½	P. M.	id.	66	39	2
24.	6½	A. M.	30·38	59	39¼	1½
	2	P. M.	30·32	66	39	4
	10½		30·3	65	id.	1½
25.	6½	A. M.	30·25	61	39¾	2½
	3½	P. M.	30·21	69	38½	3
	10½		30·28	68	39	2
26.	7	A. M.	30·23	59	40	2½

Alteration suggested.

This great diminution in the *frequency* of the *strikings*, made me think of connecting the *ball* 18 with the *negative* extremity of all the *columns*, in order to see what increase it would make in the *frequency*: but luckily at the same time it occurred to me, that, by producing a speedy manner of changing this connexion of the *ball* for that with the *ground*, and inversely, it would be a mode of discovering variations in the *electric state* of the latter, by comparing in a short time its effect on the *strikings*, with that of the *negative* extremity of the *columns*. This was the occasion of the last change which I made in the apparatus, as represented in the *figure*, which part I am now going to describe.

Addition to the apparatus.

On the lead base 19, 19, I fixed two insulating pillars 27, 27, and on these a brass piece 28, at one extremity of which is fixed the *ball* 18, and at the other the machinery for moving the horizontal *silver wire* 21. By this insulation of the parts against which the *bead* and its *silver wire* come to *strike*, I can place them in a moment in communication with either the *ground* or the *negative* extremity of the *column*, by only changing the position of a brass wire 29, hooked to the extremity of the horizontal brass piece. In the position of this *wire*, as represented in the *figure*, the *ball* 18 and the *silver wire* 21 are in communication with the *ground*; and when it is wanted to make them communicate with the *negative* extremity of the *columns*, I have only to take up the movable *wire*, and to lay its end on the projecting brass piece of that extremity. I was surprised to find so little difference of effect between the communication of the *ball* 18 with the *negative* extremity of the *columns*, and with the *ground*, which is a standard between the *negative* and *positive* states of bodies; and upon the whole this kind of observations opens a new and interesting field of researches. Therefore, though I had but a short time to follow these observations, the following journal will show at least the nature of this phenomenon:

		Barom.	Thermom.	Hydrom.	Numb of strik. in 5 min. With the ground.	Numb of strik. in 5 min. With the negat extrem. of the col.
May 26.	4 P.M.	30·15	71	38¾	3	6
	9	id.	68½	39	2	2
27.	8 A.M.	30·13	61	39¼	2	2
	11½	30·11	62½	39¼	2	3
28.	1 P.M.	30·30	65	38½	1	2
	9	30·40	66	id.	2	2½
29.	7 A.M.	30·55	56	id.	½	1
	12	id.	64	37¾	1⅝	2¼
	8 P.M.	30·53	68	37½	⅞	3
30.	7 A.M.	id.	58½	38¼	⅚	1⅕
	1¼ P.M.	30·50	66	38	4½	4
	4	id.	69½	38½	4	3¼
	9	id.	67	38¼	1¼	2
31.	7 A.M.	id.	60	38½	1	1⅖
	2 P.M.	30·45	68	id.	4	3
June	9½	30·48	69	id.	2	1½
2.	8 A.M.	30·45	63	39½	4	4
	11	30·44	64	39¼	4	4¼
	3 P.M.	30·45	69	37½	1⅔	1⅔
	9½	id.	69	38	1¼	1⅔
3.	7 A.M.	30·42	62	39	1⅔	2
	11	id.	65	38¾	2½	3
	2 P.M.	30·38	69½	38¼	4	4
4.	6½ A.M.	id.	64	38½	⅘	1¼
	10	30·40	65	id.	2	2
	4½ P.M.	30·38	69	37¾	1⅔	1⅔
5.	8 A.M.	30·45	60½	38½	1⅔	1⅔
	11	30·40	63	id.	1⅞	2⅐
	2¼ P.M.	id.	68	38	2¼	2⅘

Object of the last observations.

In the last days of these observations, I had some reason to suspect, that something had been deranged in the apparatus, but I could not examine it, as I was preparing to leave Windsor for spending the Summer in Devonshire, where I write this paper. However, the defect which I suspected did not interfere with the object of this last series of observations, which principally relates to the *electric state* of the *ground*. This *state* is here compared with that of the extremities of the *column*, which I have called *negative*, though it is sometimes *neutral* comparatively with the *electric state* of the *ambient air;* but it is never *positive*. On the other hand, the *bead* never moves but as *positive* comparatively with the same standard, and it moves the faster, as the *ball* 18 differs more from its *electric state*. Now it is seen in the above observations, that sometimes the *bead* moves faster when the *ball* is in communication with the *ground*, than when it communicates with the extremity of the *column* called *negative*. This is a test of the *electric state* of the *ground* which deserves to be deeply studied, in order to understand it better.

Were I younger, I ought not to publish these experiments and observations in their present state; I should endeavour first, to improve the instrument, in order to meet with more advantage a proper season; then to follow the motion of the *aerial electroscope* more regularly than hitherto I have been able to do, being constantly employed in improving it; and to study the connexion between these motions and the changes in the *electric state* of the *air* near the *ground*, and of the *ground* itself: a course of observations, which is to be followed from the time of the greatest effects of the *column*, to that of their rapid diminution, coinciding with the time when *vegetation*, the greatest terrestrial phenomenon, prevails on all the ground, and in which it thus appears, that the *electric fluid* has some influence. But though it is possible, that I may take up again these observations, I prefer an earlier communication to natural philosophers of the beginning of researches of this class; because at any rate these researches would advance more certainly, should they become the object of many observers, not merely for assembling scattered and unconnected phenomena, but for considering the light that they reflect upon each other, which may help to trace up their real causes. No spontaneous effects can manifest in a more characteristic manner these remote connexions between terrestrial phenomena by common causes, than those offered to our view by the *atmosphere*, in which therefore we must endeavour to extend our knowledge by meteorological

Electric state of the ground and air near it connected with vegetation.

meteorological observations: and as these phenomena have been for a long time one of the principal objects of my attention and study, I purpose to explain in the last *part* of this paper the connexion, that may exist between the indications of the *aerial electroscope*, when properly settled, and many *atmospheric phenomena*, which are daily observed, without being really understood.

Aſhfield, near Honiton,
23d August, 1810.

II.

On the Structure and Classification of Seeds. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

NOW once more trouble you on the subject of seeds, desirous of completing the task assigned to me, and finishing the sketch I began in my last. As in *that* I was careful to confine myself within those *laws*, which are applicable to the interior of the embryo; in this I shall take a different path, and show the various kinds of corculum into which all seeds are divided, and thence the various classes they might form: indicating the interior marks, which would diversify each different class, the very mechanism of which is so various, though so well defined, as to strike the mind with the appearance of a natural method; by *which means* might be established, without any difficulty, an arrangement, which would enable botanists to add them in an appropriate word to the Linnæan or any other classification preferred in giving the description of plants. Certainly, as I before observed, it is strange to give an elaborate description of every part of the exterior of the plant, even the most insignificant, and leave out the most important, the *interior of the seed*; that which is the very *essence* of it (for so the embryo may be called). In our best works of the kind no notice is taken of the heart or *cotyledons*, or of the division so well known to gardeners of leaf seeds.

Various kinds of corculum.

To

Seeds divided into five classes.

The heart the laboratory of the seed.

The miniature tree supposed to be seen in the seed.

Mechanism of the corculum unknown

Jussieu's arrangement by the cotyledons erroneous.

To explain this, and other differences, so deeply marked by the hand of nature, I have divided seeds into five classes, to which may be added orders and genera, as many as may be found necessary in future. At present I shall confine myself to a mere sketch of these five divisions, which will be but an outline to be filled up by future observation. That the heart is the laboratory of the seed, I am perfectly persuaded; for in this part begins the *whole work* of nature; from this appears to arise all its mechanical ſtrength; here are concocted, prepared, and perfected, all the various juices: in short, in this part only is seen all the variety of mechanism necessary for these purposes. I shall endeavour to prove this by a description of the various parts that compose it: which, when well dissected, and properly arranged, appear as surprising a piece of work as nature can produce. I am not in the least astonished, that physiologists imagined, that in each seed was found the epitome of many trees—for a cursory view of the corculum in a microscope might lead to such a conclusion, from the variety of figure it announces. But we are now too well informed to admit such fables. Still the mechanism of the corculum is hitherto unknown, at least *undescribed* by any author; and I flatter myself, though I cannot *explain* the *nature* of each secretion performed there, that I shall (as far as the sight can discover) *show its structure.* The present letter will receive additional interest from a discovery I have just made (even since my last letter); for I flatter myself it will complete what my first letter showed; the necessity of abandoning that arrangement of Jussieu, which is founded on the number of cotyledons. For if it can be proved, that there are no plants *without* cotyledons; that what he announced as monocotyledons were *dicotyledons*, and that what he mentioned as dicotyledons *have many*; it muſt of course be confessed, that the arrangement is erroneous, and wants correction. In my former letter I plainly proved, that the primordial leaf in the grasses and palms had been mistaken for the cotyledons.

I shall soon give a proof of this, not to be controverted, as nature herself will show it; but I may now make a farther assertion, and say, that, *except* in seed-leaves there are

few

Recess discovered in a walnut.

The walnut has from four to nine cotyledons.

few dicotyledons. Dissecting a very unripe walnut, in order to discover the course of its nourishing vessels in the corculum, I perceived a recess I had not before noticed. Desirous of knowing what it contained, I fastened a double magnifier of great power over it, so that I might scrape its interior with a fine lancet. I drew out three diminutive points, which in the solar microscope I soon found to be perfect cotyledons. I then dissected another walnut, and discovered four much larger, which covered the lower part of the corculum like a festooned curtain. These I had before seen, and taken for scales of the kernel, so assured was I, that there could be but *two cotyledons*. Hence the mischief of trusting to any person, or thing, in the *study of nature*, but herself. I then divided the corculum as usual, and found the common cotyledons: in one walnut therefore I found nine, and in above 60 more I discovered from four to seven. I doubt not many more may be seen in an older walnut. I shall now give the picture of the corculum of the walnut, which will better enable the reader to understand the description of the different classes, and of the number of cotyledons that belong to each, with all the rest of its arrangement.

Corculum of the walnut.

Plate V, fig. 1, represents the corculum of the walnut when covered by the upper colytedons: *x x* two of the colytedons taken off. Fig. 2, the corculum or heart of the walnut when divided, showing what have always been reckoned cotyledons at *c c*, and the breast with the teats *d d*. Fig. 3. the corculum turned so as to show the back. *f* The recess.

Horse-chestnut.

After opening a quantity of walnuts, it may well be imagined, that I was impatient to see whether a recess was to be found in all seeds; and if other seeds had these additional cotyledons. The first I tried was the esculus, which had exercised my patience by constant dissections for a long time, in search of them. But I had now found a clew, and discovered therefore 4; though from the peculiarly uneven formation of its pocket, it was not till after a pretty long search. I must therefore recal the declaration in my last paper, nor shall I ever fail to do so, when I find myself mistaken;

seeking for truth, I may err, but I will never deceive. Since my last discovery, I have been able only to dissect 200 seeds; but these and my former studies amount to above 2000, and will enable me to judge in what class the many-cotyledons are found. I shall now therefore begin with the description of the formation of the corculum of each different class.

1st class. Mammiferæ.

The first is the mammiferous. It includes plants of a very strong and vigorous form and nature, not only in trees and shrubs, but in smaller plants. The oak, beech, elm, horse-chestnut, &c.; the laurel, rose, budlea, &c.; burdock, sun-flower, and many of the order pentandria digynia, that are spreading and vigorous. This seed has a remarkably large heart, into which the juices are conveyed by the nourishing vessels. In the corculum is found that curious part, which resembles the breast of a bitch; with teats (as far as I have been able to discover) numerous according to the strength of the plant. Over the teats are the nourishing vessels, and so much juice do they impart to these curious forms, that the recess is often inundated with the juice that runs through them. What effect this straining may have on the liquid is easy to imagine, and that from crude and harsh it may become both sweet and emollient. It may also be more intimately mixed, and thus form in those spaces gasses suited to the object to be nourished. There is found in this species of heart from 12 to 16 teats, which bleed in three or four places. The recess is discovered at the back of the corculum, and through the middle of it passes that line, which afterward is called the stalk of the plant, and is now only the line of life; and one row of wood vessels, covered by the circular skin of cotyledons, or of that matter which forms them. See Fig. 6. This recess is the place where the cotyledons are mostly formed, and from which they branch, while the primordial leaves proceed from the interior line. This structure plainly proves, that the nourishing vessels are the feeders of both cotyledons and primordial leaves. All the fruits have a heart of this kind, and many cotyledons. Most of the carices and wheat are of this class. It is curious, that in this not only the latter but the former leaves will appear, and burst through the thickness

The seed described.

observed *over* the recess, to show their strength and vigour; this is often seen in the apricot, peach, &c.; which form in this manner two little nosegays. If a strong magnifier be directed to the top of the recess at the back of the corculum, it will show a small aperture, through which the hidrogen enters the heart. This I believe, because before this vessel is seen the heart never cracks in the fire, and the moment it is found the seed explodes.

2d class Foliferæ.

Divisible into two orders.

The seed described.

Difference between the seed leaf and common seed.

The 2d class I have named foliferous. See fig. 7. Every gardener knows the seed leaf from those seeds that are not so. It is an embryo that rises out of the earth with its cotyledons: though they do not all appear above ground; but those plants which have only two, show themselves growing up with the stem for a little time at least. This class might well be divided into two large orders. The firs with those rising plants that have many cotyledons; and those which have only *two*; but this I shall leave to a future arrangement. The second class then consists of the firs, and a number of plants that are the spontaneous growth of the soil, the pride of the fields, arenarias, stellarias, cinquefoils, euphorbias, beside all running and twining plants. This seed has a remarkably small heart, with a few points, that can hardly be called teats, though they seem to act as such, having the nourishing vessels above them. They have I believe seldom above two cotyledons, though we do know an exception to this rule in the mustard and cress, the former of which has four, the latter six. I mentioned in my last letter, that I would show the difference of growth of a seed-leaf and a common seed; that is, every seed that is not a seed-leaf. Till the end of the second epoch they exactly resemble each other in their manner of growing. The seed-leaf then, instead of shooting out its *primordial leaves*, continues to increase its *cotyledons*, which grow on in size, till they turn to leave the seed, which they do in exactly the same manner as in the other seeds. There is little difference therefore, except that the primordial leaves do not *shoot, till* they have quitted the seed. To this class belong most of those plants, which are found in the class cryptogamia of Linnæus: the lichens, the mosses, the fungi. The fungi I have not however thoroughly ascertained, and leave them for a future trial; nor have I dissected the seeds of the sea-weeds.

3d class Caniliculatæ.

The third class of seeds is a numerous one, and I have called it *The canaliculated.* See fig. 10. It is distinguished by a larger heart, with a curious sweep, which figure the teats follow. The teats are numerous, and have the nourishing vessels above them. This class takes in almost all the papilionaceous, ringent, and many of the cruciform flowers. The formation of the corculum (much as they may differ in each seed) will still be found to have the mark of this class; which is principally a deep furrow beginning with the recess, runing on to the end of the primordial leaves, and lengthening as the embryo increases. I have two or three times found cotyledons in this passage, and I am rather inclined to believe, that farther search will show more, especially in the papilionaceous, which is also distinguished by a curious sheath, that holds that jelly found constantly in the pocket of the seed, and against which the primordial leaves shoot. But I do not conceive, that more than four cotyledons will ever be found. I have never seen more.

4th class Nonmammiferous.

The fourth class is the nonmammiferous, and is the one that differs most from the rest; for it has neither *recess, nor teats.* See Pl. VI, fig. 1 and 2. The palms, and grasses, are included in this; beside many odd plants, which it would be useless in such a sketch to mention. The distinguishing marks of this class are the *cotyledons* proceeding from the upper end of the corculum, instead of the usual place; this was the reason, that in the grasses botanists overlooked them; took the *primordial leaf* for the *cotyledons,* and named them monocotyledonous. But had they dissected *the interior,* they would have found, that they are placed (with respect to the primordial leaf) *exactly* as in every other plant; both rising and branching from the same apparent source. This is sufficient to prove, that these little leaves (always given in those excellent drawings of Sowerby) are really the *cotyledons* of *the grasses*; and that they have always either two or four, as well as the palms. The class is also easily known by having the stalks running through the corculum without impediment; and the nourishing vessels protruding on one side of the heart only, which has been the cause of many mistakes concerning the radicle, which I mentioned in my last. The false grasses, (such as the *cyperus, scirpus, carex,* &c) belong to the first, as well as wheat and rye; but barley, oats, &c., to this. When my plan is more perfected *(if approved)* I hope to give a list, that will more exactly point out the arrangement.

5th class. Mixed, or compound.

The last class I have called the mixed or compound seeds. See Pl. VI, fig. 3. It includes most of the water plants, the spice, coffee, and some cotton plants. I have not yet been able to acquire foreign seeds sufficient to enable me to arrange it with the perspicuity I would wiſh; but it has notwithstanding some striking features, fully capable of marking and distinguishing it from the other classes: for it has the large and prominent heart of the first class, with the seed leaf of the second; it has many teats, and a roomy recess, for the formation of the cotyledons. I have no doubt, that many seeds I am yet unacquainted with will rank in this class. The bladder tree appertains to it, and a curious plant brought me by a gentleman from the East Indies, who was one of those engaged in the trigonometrical survey there, and who found it in the wildest part of the peninsula, that few but themselves ever crossed. I have not been able to procure Rumphius, to seek it there; and can find it only in Gerard, who calls it "arboris lanifera siliqua." Supposing it little known, I have selected it as an example of this peculiarly formed *corculum*, well marking the class, and shall describe the plant also. It has a pod six inches long, two and a half wide, full of the most beautiful cotton, weighing nearly a quarter of a pound, and having within the seed vessel a number of triangular black seeds, rounded at the edges. In dissecting this seed, a large *heart* is found, rather larger in proportion than in the first class, and having two seed leaves of great length, curled up very thick, and the intermediate part of the seed filled with a substance like flower. On stretching the cotyledons, they measured near an inch and half, and in some seeds I have found two cotyledons *above:* and in most seeds of this class there are from *four* to *six*. This tree is a large one, and has leaves very long and slender; the outside rind is thick and spongy. The flowers I have not seen, nor have I received

I received any description of them; but the cotton resembles silk, and is more beautiful than that the silk worm spins. It is said to grow also in Bantam, and to be much valued. To complete the account of the corculum and of this fifth class I shall only mention, that it has from 14 to 18 teats, with very large nourishing vessels: the long cotyledons almost wholly fill the seeds in general; and it appears to me an additional proof, that they contrive to grow as long as their room and time will admit; for seldom can there be found seeds showing a regular number of cotyledons: the longer they remain in the seed vessels, the more there are; and in this last class, the longer they are.

Number of cotyledons never regular.

Hints on dissecting plants.

I shall now give a few hints to those botanists that wish to dissect their own plants, and to judge for themselves. Patience and habit are every thing: perhaps in no particular does practice repay so amply as in this. The hand grows more delicate in the touch; and the eye so very much improves in sight, that what at first cannot be seen distinctly, with a good magnifier, will soon become plain to the naked eye. The habit of dissecting with the mouth likewise all botanists should endeavour to learn, for no instrument can act like it, or so thoroughly divest the seed of all superfluous parts, and prepare it for the microscope.

Rules for distinguishing the classes.

As to the rules for distinguishing these classes, without obliging any person to repair to the solar or other powerful microscope: the first class is easily known by a small magnifier, but the second requires some art. They are generally remarkably small seeds; press them between the nails of your thumb, beginning the pressure at the corculum end, and the whole embryo will slip out *heart and all*; you have then only to divide it with a fine lancet. The third class must not be so tried, but lay it straight on your seed hammer; and pressing a flat knife on it, pass your lancet between, and it will always divide it *exactly* as it should do, showing the two principal vessels in a manner that will teach much, for this class of seeds is one of the best to begin dissection by, as there is no confusion in the arrangement of the vessels. They are at such a distance from each other, that it is hardly possible to mistake them. I have drawings of a large size of many of this class, which are of great use in showing the formation and habit of a seed, and teach more (if well studied) than any other: the 4th and 5th classes are large enough to be dissected with the usual instruments. But for the diminutive seeds larger powers are required, as the powdered lichens, fungi, mosses of the smallest kind, &c. It is best to keep these till *very* ripe, then place the seeds in the several sliders of the solar or double microscope, and you will always find two or three opened sufficiently by the heat and light, to draw the figure of the interior, if extremely magnified.

Cambium or albumen.

I shall now conclude the present letter with the explanation of a term that has long demanded attention, particularly on the subject of seeds, which it concerns greatly. I mean the word albumen or cambium, a matter found wherever new wood is to be created. Duhamel calls it *cambium*; Mirbel "la substance organisatrice," and gives this description of it: "Soit que les fluides y développent "par leur impulsion les cellules, et les tubes; soit qu'une "puissance inconnue, y agisse seule et y détermine ces "développements; soit, comme il est probable, que ces "deux causes combinées, y agissent de concert pour changer en tissu membraneux la substance organisatrice, "&c." I cannot think this is described with his usual perspicuity, for he does not even show what it really is. Mr. Knight makes it much more plain, but thinks it proceeds from the bark. Much as I have profited by his remarks, which always carry with them the conviction, that he has *deeply studied the subject*, I cannot agree with him in this opinion. I have perpetually seen it grow on the dry piece of a seed vessel, which I have placed *within* the *graft* for the purpose; in the same manner I have put the edge of a knife, and a diminutive piece of muslin, and found the cambium growing on them, as on the wood, and bark. Now whence does this substance proceed? from the juices of the plant alone, from the mixture of the sap with the blood of the plant, resting on the part, and there forming as a crystal, since like a crystal it is the produce of the joint juices. But it is very different from the jelly found in the pocket, which also has been improperly called albumen. I shall now give an account of it, describing its appearance

rown on the Structure & Classification of Seeds.

Fig 1

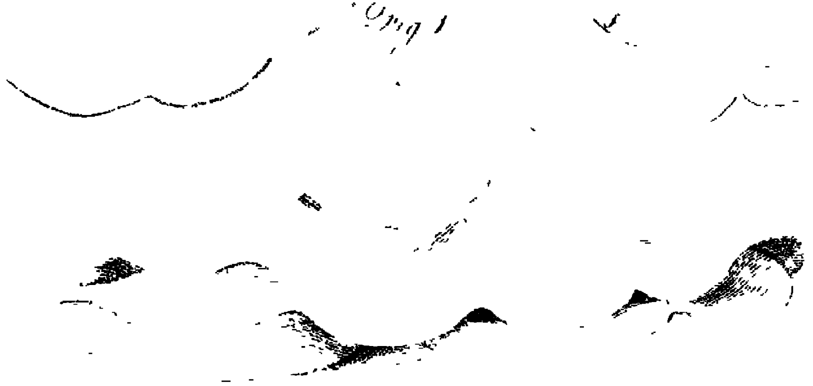


Fig 2

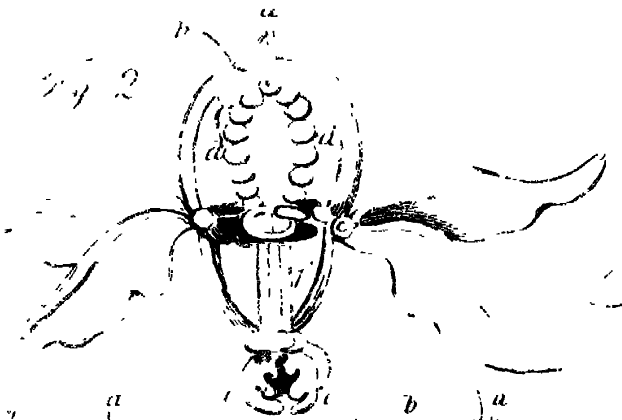
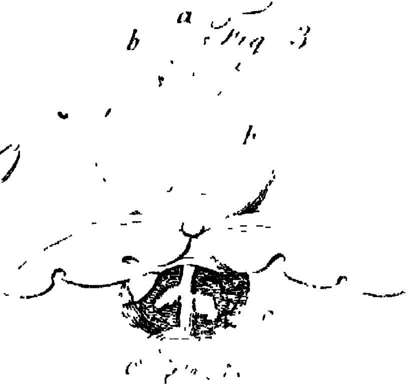


Fig 3



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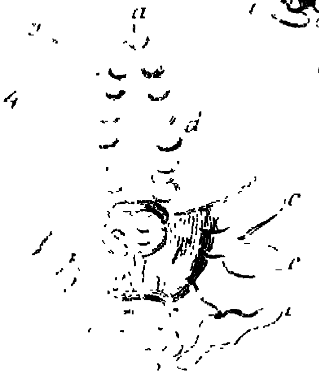


Fig 5

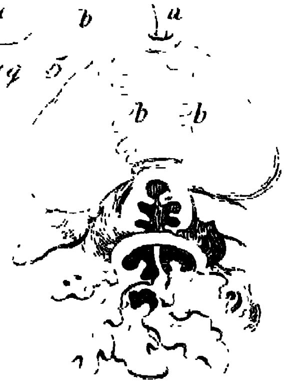


Fig 6

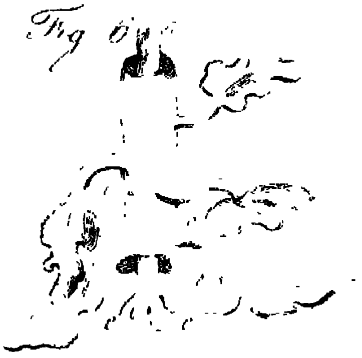


Fig 7

Fig 9

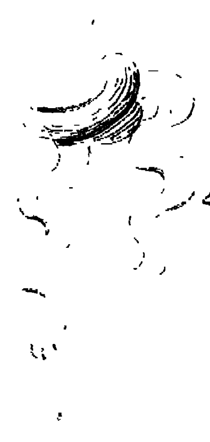
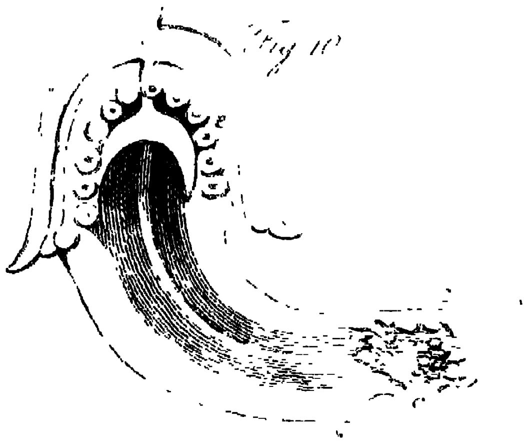


Fig 10



pearance in the solar microscope. It is all composed of extremely diminutive netted bags, of thick juice, without any vessels—in short, it is the first formation of the pabulum or softer part of the wood; and when ready prepared for the sap vessels, they shoot their way through this soft substance. In a graft, which I have repeatedly tied up again, *before* the vessels had begun to appear, and when I reopened it, I found them and the wood perfectly complete. I have taken this matter from a graft, from a fresh budded plant; from the *interior of a seed,* and sometimes from the shooting of the fresh line of the wood, but this is generally too hasty a performance to profit by; the fresh wound of a tree is the best way of getting it (next to a graft) if well preserved from the air, and in a fortnight plenty will be found. But the specimen must be quickly taken, or the wood vessels will shoot. This is the true cambium, the softer part of the wood, before the sap vessels shoot. But I must notice, that the bark is not made in the same manner: it is formed all at once, soft and hard; the vessels shoot, while the rest is forming. Mr. Knight very properly observes, that in a graft the fresh wood always resembles exactly the wood of the graft, and not the stock.

How obtainable.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

The five classes into which I have divided the seeds.

Classes of seeds.

Common seed, or first class.

Mammiferous, or teat-bearing. See walnut, Pl. V, figs. 1, 2, 3; apricot, figs. 4, 5, 6.	Oak, elm, beech, horse-chestnut, &c.; rose, laurel, budlea, &c.; burdock, sunflower, and many other compound flowers,

Second class,

Leaf-bearing or foliferous. See figs. 8 and 9, showing the whole embryo, when forced out of the seed in the manner described above; and fig. 7, the heart, or corculum, alone,	Firs and spontaneous plants of the soil, as arenarias, stellarias, potentillas, euphorbias, and many of the running plants, &c,

Third

Third class.

Caniculated, or channelled, so called, from a channel, which begins within the recess, and runs on beyond the primordial leaf. See fig. 10, the upper part representing the corculum; the lower, the whole of the embryo together.	A numerous class, containing most of the papilionaceous, cruciform, and labiate plants.

Fourth class.

Nonmammiferous, having no teats, and no recess: distinguished also by having the primordial leaves as well as the cotyledons at the head of the corculum. See Pl. VI, figs. 1 and 2.	Grasses and palms.

Fifth class.

Compound or mixed seed. See Pl. VI, fig. 3.	Nymphea, coffee, some spices, and cotton tree.

Fig. 6 is merely to show the manner in which the stalk, *n*, runs through the corculum; the primordial leaves, *e e*, being within; the cotyledons, *c c*, shooting from the outward cylinder.

In all the figures the same letters of reference are used. *a*, the line of life, or impregnating duct. *b*, nourishing vessels. *c*, cotyledons. *d*, the breast and teats. *e*, primordial leaves. *f*, the recess. *n*, the stalk.

Mr. Ibbetson on the Structure & Classification of Seeds

Fig 1

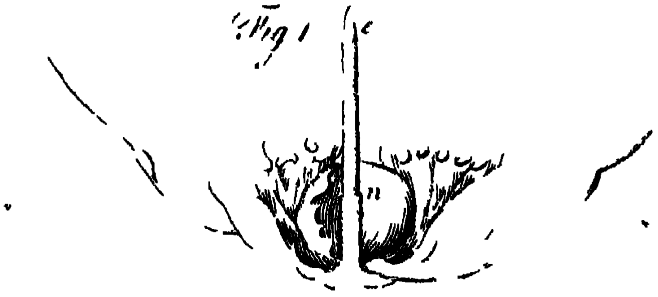


Fig 2

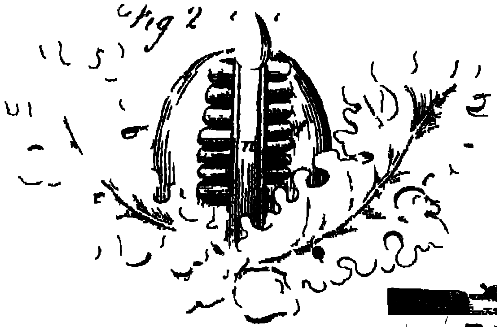
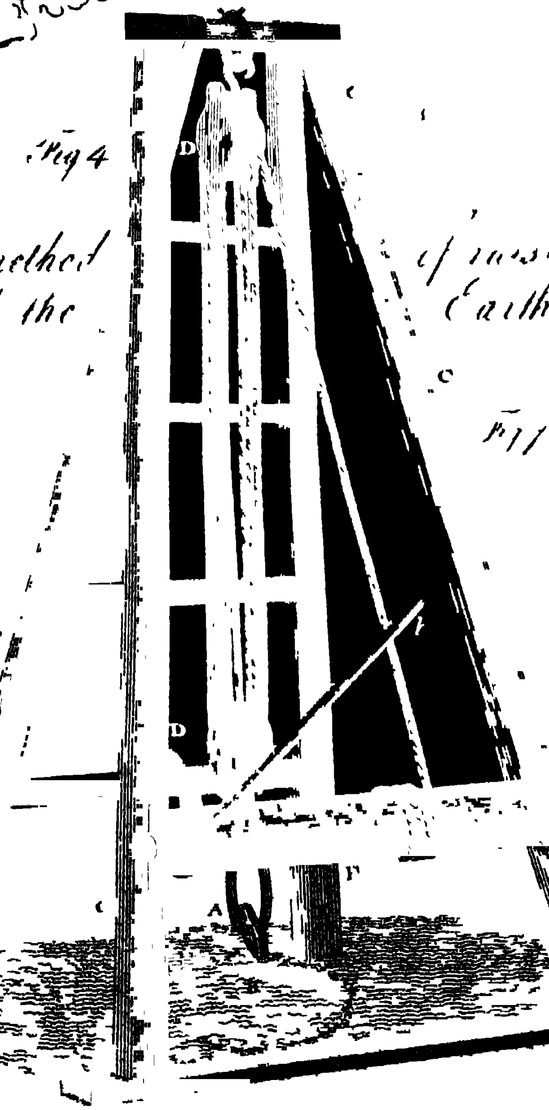


Fig 3

Fig 4



Mr Richardson's method of raising large Stone out of the Earth

Fig 5

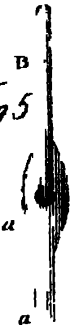


Fig 6

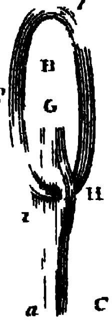
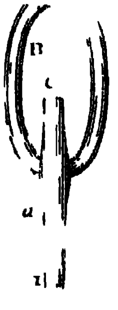


Fig 7



III.

Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.*

(Continued from p. 144.)

Observations on the Tables respecting the Thinning of Woods, and their Produce.

MR. Salmon is the only person I know of, who has given a general rule for thinning plantations. But as I conceive his distance of one fifth of their height would leave oaks too close, especially after they had acquired a sufficient length of stem, I have calculated both on his plan, which is proper for fir trees, and also at greater distances.

One fifth of their height too close for oaks,

not for firs.

The preceding Tables VI, VII, and VIII, are calculated on a supposition, that the trees are never suffered to stand nearer, on an average, than one fourth of their height; and although the quantities of timber thinned out and left standing on the ground at that distance, at the end of 60 years, is only two thirds of the quantity according to Mr. Salmon's distance, yet I suppose it will be generally thought an ample produce, and sufficiently encouraging.

Tables calculated at one fourth.

According to Table VI, which is calculated for oaks, the first thinning is at sixteen years old, and the second at twenty; but it is the advice of an eminent planter, (Mr. Poutey,) to begin thinning at about thirteen years old, according to the state of the trees, and to cut out about 150 poles per acre annually, for the next seven years. Without putting any value upon the thinnings before 20 years old, we find that at the 20th and 24th years, the thinnings measure 945 feet, the value of which, at a low estimate, will be sufficient to repay the rent and taxes of ground of a moderate quality, with the expense of plants, planting, and after-management, calculated at 5 per cent compound interest.

Thinning of oaks.

Profit of thinnings after 28 years.

When 28 years old, and at the end of every fourth year following, up to 120, the trees to be cut out of an acre will measure from 495 to 550 feet; but say 500, at 4*s.* a foot, on an average, including the value of the bark; this gives 100*l.*; which sum, divided by 4, leaves 25*l.* for the produce per acre per annum. This deserves the consideration of those who are inclined to convert young woods into coppices, without leaving a reasonable number of standards.

It may however be said, that, as the trees cut out in thinning plantations are the bad thrivers and underlings, their contents will be less than the average; but, if we take their value at one half the above estimate, that is, after the rate of 12*l.* 10*s.* per acre per annum at 28 years of age and upwards, even this produce must be thought ample, together with the value of the trees left standing.

Table for oaks.

Table VI was constructed chiefly with a view to oaks, their annual increase in circumference varying from ¾ of an inch to 1¼ inch, the medium of which is 1 inch.

Tables for fast growing trees

Table VII and VIII were calculated for ash, elm, sycamore, firs, poplars, and other woods of swift growth, their increase in circumference being generally from 1½ to 2 inches annually. If ash trees be found to increase after the rates of Table VII, or VIII, they must be exceedingly profitable, at the high prices now given for that timber. Many other observations might be made on Tables VII and VIII, but these will readily occur to persons interested in quick growing trees.

Differences in the tables.

An acre of trees, increasing after the rate of Table VI, produces in 64 years little more than half the number of feet, that another acre produces, which increases after the rate of Table VII; and little more than one third of another, increasing after the rate of Table VIII, in the same time.

Advantage of thick planting,

In planting with a view to profit, the first object is a long, straight, and clear stem. This is most certainly and speedily obtained by thick planting at first, and not thinning too soon. A kind of competition among the trees is thereby occasioned, each struggling, as it were, to outgrow its neighbour, in search of light, heat, air, and moisture.

This

and timely thinning.

This competition must, however, be judiciously moderated by timely thinning; always keeping the trees sufficiently strong in the stem. If they be suffered to stand some years too near each other, their stems will become weak, and bend under their small tops when thinned. Where this has taken place in only a small degree, they will make but little progress for some years afterward.

After a time strong side branches necessary.

By the time the trees have advanced to 24 or 30 feet high this competition should cease, if they are intended to be cut down at or before 60 years of age; and they should then be encouraged to extend their tops more in width than in height, strong side branches being apparently quite as conducive as the leading shoot, to the vigorous growth of the bole below them. After this period, the best rule for thinning will probably be, to leave a clear space around the top of each tree, in which the branches may extend themselves without obstruction. A tree the top of which is 20 feet diameter, receives four times the benefit from air, rain, and dew, that another does, the top of which is only 10 feet diameter.

Outer trees of a wood largest.

The trees in the interior of young woods are smaller in their boles than the exterior trees. And in a fine oak wood, of about 40 acres, divided into squares by several avenues or ridings crossing each other at right angles, I observed the rows of trees next the avenues much thicker in their boles than the trees in the interior of the squares; owing, no doubt, to their having more and larger branches in consequence of their having more room, although it is only on one side.

Advantage of thin woods.

Being too parsimonious of ground seems to me a great and very general errour. If the same number of trees of 32 feet high and upwards, in Table VI, were allowed the space of two acres instead of one; and, in consequence of their standing thinner, were to increase annually only the fiftieth part of an inch more in girt, than they would do if they stood on one acre, this small additional increase in girt would pay an ample rent for the additional acre.

In the year 1791 a paper of Observations on the Propagation and Management of Oak Trees in general, but more particularly applying to his Majesty's New Forest in Hampshire, was published by T. Nichols, Purveyor of the Navy for Portsmouth Dock-yard.

In this paper it is said, that "there are to be seen in many parts of the forest from 40 to 50 fine oaks standing on an acre, that will measure one with another two loads a tree."

Injury from neglecting to thin.

"Several woods in the forest are almost ruined for want of thinning, and it's being done at proper times; particularly the enclosures that were made in the year 1700;—these were originally well planted, and great numbers of trees brought up in them, which now remain so close together, that they are nearly stagnated, particularly in Salisbury, Trench, Brumley Coppice, and Woodfidley; and, although it is 90 years since they were planted, the trees will not measure, one with another, above six or seven feet a tree; whereas, if the business of thinning had been done as it ought, the remaining trees (after drawing much useful timber) would by this time have been of a size nearly fit for naval uses; as in some of the woods, that were planted at the same time, the trees which have had room to expand, and a free air admitted to them, will measure from 70 to 80 feet."

Observations on the Growth of Timber.

Timber grows thickest on the side next the sun.

The rings observable in the transverse section of a tree at its but-end are the same in number as the years of its age; an additional ring being produced annually, in consequence of the annual rising of the sap. The rings are nearly concentric in trees that have grown in the interior of close shady woods, but eccentric in others, being of different breadths on the northern and southern sides of such as have grown single, or in any other situation, where their boles have been much exposed to the rays of the sun. This difference is occasioned by the different degrees of heat, to which the opposite sides of the boles of trees are exposed. And, indeed, we find these rings are always broadest on that side of the bole or stem most warmed by the sun. Hence we see the utility of exposing their boles as much as possible to its rays*. It is often seen in the stumps of trees that have stood single, that they have grown nearly twice as fast on the southern side as on the northern, their pith being so much nearer to the northern side.

But the wood is not so heavy.

It is, however, to be remarked, that the wood from that side of a tree, which has grown the slowest, is heavier than from the opposite side, which has grown the fastest; and it is probably stronger in the same degree.

Plantation of southern slopes.

It may be worth the consideration of those, who have southern hangs or declivities to plant, whether to plant, or rather leave the trees in thinning, in double rows in lines running east and west, at about fourteen or sixteen feet distance, and the double rows at about thirty-six feet distance, less or more, according as the declivity is more or less, in order that their boles may receive the greatest possible benefit from the direct rays of the sun.

Facts respecting the action of the sun on trees wanted.

No doubt many gentlemen are in possession of facts, that would in some degree ascertain how much faster the boles of trees swell, that stand exposed to receive the full benefit of the warmth of the sun, than those that are either partially or constantly in the shade. To make these facts known would materially benefit planters; for I am fully persuaded, that there are but few persons apprised of the magnitude of the power of the sun's rays upon the boles of trees in causing them to swell.

Of the most profitable Length of Boles of Trees.

Cautions on pruning forest trees.

We rarely see timber trees pruned, and still more rarely do we see the pruning performed in a judicious manner. This business should commence early, never suffering the branches on the intended stem or bole to grow to a large size, in order that, when cut off, the wounds may be small and soon healed. Those who want directions for performing the operation may think well to consult Mr. Pontey's Forest Pruner. There are, however, divers opinions as to the

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Difference of heat on the two sides of a tree.

* On a hot day in the middle of May I have observed the mercury in the thermometer to rise and fall from twelve to sixteen degrees, on hanging it alternately on the sunny and shady sides of the same tree, between the hours of two and five o'clock, at which time of the day the heat is generally the greatest.

most profitable height, to which trees ought to be pruned, and the instruments most proper for pruning; some persons objecting to the use of the saw, unless afterward smoothed by the knife; and not a few objecting to pruning in any way; the consequence of which is, that we often find trees that stand single, particularly oaks, with boles not more than six or eight feet high, but with wide spreading bushy tops, fit only for the fire. The shade and drip of one such tree is sometimes found to do more injury than four well-trained trees, and perhaps it is not of half the value of one of them. On the contrary, trees in close plantations are often suffered to stand so much too thick as to destroy each other's branches, excepting only a few small ones near their tops; and not unfrequently we see tall elms trimmed up to within a few feet of their summits—it is certain, that such trees must swell very slowly in their boles; for we find in woods where the trees are all of the same age, that those with the largest tops have generally the thickest boles.

Proper length of bole.

There is no doubt a medium length of bole for different kinds of trees on different soils, that will be found productive of more timber, or timber of more value, than boles that are much longer, or much shorter. And although we may not be able previously to decide with certainty what that exact length of bole is, in any kind of trees, on any soil, which will eventually prove most profitable, yet it is deserving of investigation, if we can thereby approach with certainty to within a few feet of the exact point. It is certainly a matter of too much importance to be left, as it generally is, to each individual woodman to decide upon, according to his own vague opinion. I shall, therefore, take the liberty of stating by what steps I have endeavoured to approximate towards the most profitable lengths of boles of trees of different rates of growth, that are not intended to stand beyond the age of sixty years.

In the preceding tables the trees are supposed to be measured to the top of the leading shoot, but in the following tables only to the height of their boles of 24, 32, and 40 feet.

TABLES *showing the Increaſe of* BOLES *of* TREES *of different Lengths.*

Table 10.
On the length of boles.

If a tree has increased twelve inches in height and one in circumference annually, until it is twenty-four years old, it will then be twenty-four feet high, and three inches girt at twelve feet high; and supposing, that in process of time this tree is pruned up so as to leave the bole twenty-four feet high clear of branches, and that it continues increasing one inch in circumference annually; the rate per cent per annum of its increase will be as under, exclusive of the increase of timber in its top and lateral branches.

TABLE X.

Years old.	Girt.	Contents			Years old.	Girt.	Contents.			One year's increase.			Increase per cent per ann.
	in.	ft.	in.	p.		in.	ft.	in.	p	ft.	in	p.	
24	3	1	6	0	25	3¼	1	9	1	0	3	1	17·1
28	4	2	8	0	29	4¼	3	0	1	0	4	1	12·7
32	5	4	2	0	33	5¼	4	7	1	0	5	1	10·1
36	6	6	0	0	37	6¼	6	6	1	0	6	1	8·4
40	7	8	2	0	41	7¼	8	9	1	0	7	1	7·2
44	8	10	8	0	45	8¼	11	4	1	0	8	1	6·3
48	9	13	6	0	49	9¼	14	3	1	0	9	1	5·6
52	10	16	8	0	53	10¼	17	6	1	0	10	1	5·04
56	11	20	2	0	57	11¼	21	1	1	0	11	1	4·5
60	12	24	0	0	61	12¼	25	0	1	1	0	1	4·1
64	13	28	2	0	65	13¼	29	3	1	1	1	1	3·8
68	14	32	8	0	69	14¼	33	10	1	1	2	1	3·5
72	15	37	6	0	73	15¼	38	9	1	1	3	1	3·3
76	16	42	8	0	77	16¼	44	0	1	1	4	1	3·1
80	17	48	2	0	81	17¼	49	7	1	1	5	1	2·9
84	18	54	0	0	85	18¼	55	6	1	1	6	1	2·7
88	19	60	2	0	89	19¼	61	9	1	1	7	1	2·6
92	20	66	8	0	93	20¼	68	4	1	1	8	1	2·5
96	21	73	6	0	97	21¼	75	3	1	1	9	1	2·3
100	22	80	8	0	101	22¼	82	6	1	1	10	1	2·2
120	27	121	6	0	121	27¼	123	9	1	2	3	1	1·8
140	32	170	8	0	141	32¼	173	4	1	2	8	1	1·5
160	37	228	2	0	161	37¼	231	3	1	3	1	1	1·3

But

Table 11. On the length of boles.

But if a tree increase 12 inches in height and one inch in circumference annually, until it is 32 feet high, and in process of time the bole be pruned up to this height, the rate per cent per annum of the increase of this bole will be as under, exclusive of the increase in its top and lateral branches.

TABLE XI.

Years old	Girt.	*Contents.			Years old.	Girt.	Contents.			One year's increase.			Increase per cent per ann.
	in.	ft.	in.	pts.		inch.	ft.	in.	pts	ft.	in.	pts.	
32	4	3	6	8	33	4½	4	0	2	0	5	6	12·9
36	5	5	6	8	37	5½	6	1	6	0	6	10	10·25
40	6	8	0	0	41	6½	8	8	2	0	8	2	8·5
44	7	10	10	8	45	7½	11	8	2	0	9	6	7·3
48	8	14	2	8	49	8½	15	1	6	0	10	10	6·3
52	9	18	0	0	53	9½	19	0	2	1	0	2	5·6
56	10	22	2	8	57	10½	23	4	2	1	1	6	5·06
60	11	26	10	8	61	11½	28	1	6	1	2	10	4·59
64	12	32	0	0	65	12½	33	4	2	1	4	2	4·2
68	13	37	6	8	69	13½	39	0	2	1	5	6	3·88
72	14	43	6	8	73	14½	45	1	6	1	6	10	3·6
76	15	50	0	0	77	15½	51	8	2	1	8	2	3·36
80	16	56	10	8	81	16½	58	8	2	1	9	6	3·1
100	21	98	0	0	101	21½	100	4	2	2	4	2	2·39
120	26	150	2	8	121	26½	153	1	5	2	10	9	1·92

Table 12. On the length of boles.

But if a tree increase 12 inches in height and one inch in circumference annually, until it is 40 feet high, and in process of time the bole be pruned up to this height, the rate per cent per annum of the increase of this bole will be as under, exclusive of the increase in its top and lateral branches.

TABLE XII.

Years old.	Circ.	Contents.			Years old.	Circ.	[illegible]			[illegible]			Increase per cent per annum.
	inch	ft	in.			inch.	ft.	in.	pts.	ft.	in.	pts.	
40	6	6	11	4	41	6	7	7	10	0	8	6	10·2
44	6	10	9	6	[illegible]	[illegible]	10	10	[illegible]	0	10	[illegible]	8·47
48	7	13	7	4	49	7	14	7	[illegible]	0	11	10	7·2
52	8	17	[illegible]	4	[illegible]	[illegible]	18	10	[illegible]	1	1	[illegible]	[illegible]
56	9	22	6	9	57	[illegible]	23	[illegible]	[illegible]	1	3	[illegible]	[illegible]
60	10	27	9	4	61	10	29	[illegible]	[illegible]	1	4	[illegible]	[illegible]
64	11	[illegible]	[illegible]	[illegible]	65	11	35	1	10	1	6	[illegible]	[illegible]
68	12	40	[illegible]	[illegible]	69	[illegible]	41	[illegible]	[illegible]	1	[illegible]	[illegible]	[illegible]
72	13	46	11	4	73	[illegible]	48	[illegible]	[illegible]	1	9	[illegible]	[illegible]
76	14	54	[illegible]	4	77	[illegible]	56	4	[illegible]	1	11	[illegible]	[illegible]
80	15	62	6	[illegible]	81	[illegible]	64	7	[illegible]	2	1	[illegible]	[illegible]
100	[illegible]	111	1	4	101	[illegible]	113	10	[illegible]	2	9	6	[illegible]
[illegible]	[illegible]	173	7	4	121	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	10	[illegible]

(To be concluded in our next.)

IV.

Demonstration of a curious Numerical Proposition, by Mr. P. Barlow, *of the Royal Military Academy, Woolwich.*

Proposition.

No power but the square divisible into two of the same denomination:

THE *equation*

$$x^n + y^n = z^n$$

is always impossible, either in INTEGERS *or* FRACTIONS, *for every value of n greater than 2.*

a proposition of Fermat's.

This theorem is one of the most interesting in the theory of numbers, both on account of its simplicity and generality, and the celebrity of those mathematicians, who have attempted its demonstration. The theorem itself is due to Fermat, who first proposed it as a challenge to all the English mathematicians of his time; he also introduces it in a note at page 61 of his edition of Diophantus in the following terms.

"*Cubum autem in duos cubos, aut quadrato quadratum in duos quadrato quadratos, et generaliter nullam in infinitum ultra quadratum potestatem in duos ejusdem nominis fas est dividere, cujus rei demonstrationem mirabilem sane detexi. Hanc marginis exiguitas non caperet.*"

who never published the demonstration.

From which it appears, that he was himself in possession of the demonstration*, though it was never published, nor has any mathematician since his time been able to restore it, notwithstanding they have succeeded in demonstrating many other of his propositions.

Euler demonstrated it in two cases,

Euler was, I believe, the first who undertook this task, and succeeded in demonstrating the impossibility in the two cases $n = 3$, and $n = 4$; that is, that the equations $x^3 \pm y^3 = z^3$ and $x^4 \pm y^4 = z^4$ are impossible, and the same two cases have also been demonstrated upon similar principles by Waring, in his *Meditationes Algebraicæ*, and by Legendre, in his *Essai sur la Théorie des Nombres*, the latter author concluding his chapter by the following remark.

as did Waring and Legendre

"*Nous avons démontré dans ce paragraphe, que l'équation* $x^3 \pm y^3 = z^3$ *est impossible, ainsi l'équation* $x^4 \pm y^4 = z^2$, *et à plus fort raison* $x^4 \pm y^4 = z^4$. *Fermat a assuré de plus (Ed. de Dioph. page 61) que l'équation* $x^n \pm y^n = z^n$, *est généralement impossible, lorsque n surpasse 2; mais cette proposition, passé le cas* $n = 4$, *est du nombre de celles que restent à démontrer, et pour lesquelles les méthodes que nous*

Mistake in a note to the translation of Euler's Algebra.

* I ought here to correct an error, that I fell into in one of the notes to the second English edition of Euler's Algebra, where in mentioning this theorem, I have said, "and the truth of it still rests on no other foundation than the bare assertion of Fermat, who probably had never demonstrated it himself." I was led into this expression by writing the note in question from memory only, not recollecting at the same time the concluding part of his sentence, where he so positively asserts his being in possession of the demonstration: and as it was by no means my intention to impeach the veracity of this distinguished geometer, I ought in justice to his memory to correct the mistake.

venons d'exposer paroissent insuffisantes." (*Page* 410, *Essai sur la Théorie des Nombres.*)

This therefore being the state of the proposition, the following general demonstration, for every possible integer value of n, greater than 2; will not, it is presumed, be unacceptable to the lovers of this interesting branch of analysis; before entering upon which, however, at length, it will be proper to make a few preliminary observations on the general equation, in order to render the demonstration as simple and concise as possible.

Observations on the general equation.

1. In demonstrating the impossibility of the equation $x^n \pm y^n = z^n$, it will be sufficient to consider n as a prime number. For suppose n be not a prime, but equal to the product of two or more prime factors, as $n = pq$, then the equation becomes $x^{pq} \pm y^{pq} = z^{pq} = (x^p)^q \pm (y^p)^q = (z^p)^q$, being a similar equation, in which the power q is a prime number; and, therefore, if the equation be possible when n is a composite number, it is also possible for a prime power; and conversely, if the equation be impossible when the power is a prime, it is also impossible for every composite power; we shall therefore in what follows consider n as a prime number.

2. We may always suppose x, y, and z as prime to each other; for it is evident, in the first place, that two of these numbers cannot contain a common divisor, unless the third contains the same. Suppose, for example, that x^n and y^n contained any common divisor, as ϕ, and that z^n did not contain the same, then, in the equation $x^n \pm y^n = z^n$, we should have $x^n \pm y^n$ divisible by ϕ, but the equal quantity z^n not divisible by it, which is absurd; and the same may be shown if any other two of these quantities are supposed to have a common divisor which the third has not. And if they have all three the same common divisor, as $x = \phi \acute{x}$, $y = \phi \acute{y}$, and $z = \phi \acute{z}$, then the equation becomes $\phi^n \acute{x}^n \pm \phi^n \acute{y}^n = \phi^n \acute{z}^n$, or, dividing by the greatest common divisor, $\acute{x}^n \pm \acute{y}^n = \acute{z}^n$: if, therefore, the equation $x^n \pm y^n = z^n$ be possible, when x, y, and z, have a common divisor, it is also possible after being divided by that common divisor,

and in which latter equation the three resulting quantities, x, y, and z, are prime to each other; and, conversely, if the latter be impossible, the former is impossible also; we shall therefore only consider the case in which x, y, and z, are prime amongst themselves.

3. It will be sufficient to consider the ambiguous sign $\pm$ under either of its forms + or −; for if the equation $x^n + y^n = z^n$ be possible, so also is the equation $z^n - y^n = x^n$; and if the equation be impossible under the latter form, it is likewise impossible under the former.

We shall therefore limit our demonstration to the equation $x^n - y^n = z^n$, in which n is a prime number, and x, y, and z, numbers prime to each other: the impossibility of which, from what is shown above, involves with it the impossibility of the general equation $x^n \pm y^n = z^n$, when x, y, and z, are any numbers whatever, and n any number except 2, or some power of 2. Now with regard to $n = 2$, we know, that the equation is not impossible, and the case of $n = 4$ has been demonstrated to be impossible by Euler, and others; and this latter case involves that of every higher power of 2, thus $x^8 \pm y^8 = z^8 = (x^2)^4 \pm (y^2)^4 = (z^2)^4$; which being impossible in the latter form, is necessarily so in the former: and in the same manner, the impossibility of the equation for any higher power of 2, may be shown to be involved in that of $n = 4$: it is evident, therefore, that our equation, together with that of $n = 4$, involves every possible value of n greater than 2.

Lemma 1.

Lemma 1. If there be two fractions, as $\frac{a}{A}$ and $\frac{b}{B}$, each in its lowest terms, and of which the denominator of the one contains any factor not common with the denominator of the other; then, I say, that neither the sum nor difference of those fractions can be equal to a complete integer number.

Let $\frac{a}{A}$ and $\frac{b}{B}$ be any two fractions in their lowest terms, so that a is prime to A, and b prime to B; also suppose B to contain a factor t, as $B = tB'$, which is not contained in A; then I say, that $\frac{a}{A} \pm \frac{b}{tB'} = n$ an integer, is impossible.

For $\frac{a}{A} \pm \frac{b}{tB'} = \frac{atB' \pm bA}{AtB'}$ which cannot be an integer, because if it were, the numerator $atB' \pm bA$ would be divisible by the denominator AtB', and consequently by any one of the factors of that denominator, as t; but the numerator $atB' \pm bA$ is not divisible by t. For since the first term atB' is divisible by t, the second term bA must also be divisible by t, if the whole quantity was so; but bA is not divisible by t, (Euclid. 26, 7) because both b and A are prime to t by the supposition; since, then, atB' is divisible by t, but bA not divisible by it, therefore the whole quantity $atB' \pm bA$ is not divisible by t, and consequently $\frac{atB' \pm bA}{AtB'}$ or $\frac{a}{A} \pm \frac{b}{tB'}$ cannot be equal to an integer. Q. E. D.

Cor. In the same manner it may be shown, if there be any number of fractions, each in their lowest terms, as $\frac{a}{A}$, $\frac{b}{B}$, $\frac{c}{C}$, &c., and of which one of the denominators contain a factor that is not common to all the other denominators, that neither the sum nor difference of those fractions, any how combined, can form an integer; that is, $\frac{a}{A} \pm \frac{b}{B} \pm \frac{c}{C} \pm \&c\ \frac{q}{Q} = n$ an integer, is impossible.

Lemma 2.

If any power of a number A, as A^n, be divisible by any other number r, once, and after that, neither by r, nor by any factor of r, then will r itself be a complete nth power. Lemma 2

First if A be a prime number, then A^n can only be divided by A, or some power of A, that is r must be some power of A; but if A^n be divided by any power of A less than A^n, it is evident that the quotient will still be divisible by by A; but by the supposition the quotient is not again divisible, either by r, or by any factor of r, therefore r must in this case be equal to A^n.

Again, if A be not a prime number; let it be resolved into its prime factors, as $A = a\,b\,c$, &c., or $A^n = a^n\,b^n\,c^n$, &c. Now if $A^n = a^n\,b^n\,c^n$, &c., be divided by any power of a less than a^n, the quotient will be again divisible by a; and in the same manner if it be divided by any power of b less than b^n, the quotient will still be divisible by b; and so on of any other divisor that is not a complete nth power; and, therefore, conversely, if A^n be divisible by any other number r, once, and after that, neither by r, nor by any factor of r, then must r itself be a complete nth power.

Q. E. D.

Lemma 3.

Lemma 3 In the expanded form of the binomial $(p + q)^n$, when n is a prime number, each of the coefficients, except those of the first and last terms, are divisible by n.

For each of the coefficients is of the form

$$\frac{n\,.\,(n-1)\,.\,(n-2)\ \&c.\ (n-r)}{1\,.\,2\,.\,3\ \&c.\ \ r+1}$$

which is always an integer, from the nature of the binomial; and since n is a prime number it is not divisible by any of the factors in the denominator, which are all less than n, except in the coefficient of the last term, which does not enter into our consideration, the coefficients of the first and last term being excepted in the proposition.

Since then

$$\frac{n\,.\,(n-1)\,.\,(n-2)\ \&c.\ n-r}{1\,.\,2\,.\,3\ \&c.\ \ r+1}$$

is an integer, and n is prime to all the factors in the denominator, therefore,

$$\frac{(n-1)\,.\,(n-2)\ \&c.\ (n-r)}{2\,.\,3\ \&c.\ \ r+1} = e$$

is also an integer, and consequently

$$\frac{n\,.\,(n-1)\,.\,(n-2)\ \&c.\ (n-r)}{1\,.\,2\,.\,3\ \&c.\ (r+1)} = n\,e;$$

that is, each of the coefficients, except those of the first and last term, is of the form $n\,c$, and is therefore divisible by n.

Q. E. D.

Cor. We may therefore in all cases, where brevity requires it, write Corollary.

$$(p \pm q)^n = p^n \pm n p^{n-1} q + n a p^{n-2} q^2 \pm n b p^{n-3} q^3, \text{ \&c.}$$
$$\pm n p q^{n-1} \pm q^n,$$ n being an integer prime number.

Proposition I.

If the equation $x^n - y^n = z^n$ be possible, then one of the four following conditions must obtain; viz., Proposition 1.

$$\text{1st.} \begin{cases} x - y = r^n \\ x - z = s^n \\ y + z = t^n \end{cases} \qquad \text{2d.} \begin{cases} x - y = n^{n-1} r^n \\ x - z = s^n \\ y + z = t^n \end{cases}$$

$$\text{3d.} \begin{cases} x - y = r^n \\ x - z = n^{n-1} s^n \\ y + z = t^n \end{cases} \qquad \text{4th.} \begin{cases} x - y = r^n \\ x - z = s^n \\ y + z = n^{n-1} t^n \end{cases}$$

where r, s, and t, may represent any numbers whatever, indicating only, that $(x - y)$, $(x - z)$, $(y + z)$, &c., are complete nth powers, or that they are of the form r^n, s^n, t^n, &c.: which proposition I first demonstrated in the 2d edition of the English translation of Euler's Algebra.

Now from what we have before observed, x, y, and z, may be considered as prime to each other, and since $x > y$, make $x = y + r$, then since x is prime to y, r must also be prime both to x and y; for if y and r had a common measure, x must have the same, because $x = y + r$, and if r and x had a common measure, y must have the same, since $x - r = y$; therefore, as x is prime to y, r is prime to both x and y. Now substituting for x our equation becomes Demonstration.

$$(y + r)^n - y^n = z^n,$$

from the developement of which, and substituting for the coefficient of $(y + r)^n$

1, n, $n\,a$, $n\,b$, &c., $n\,a$, n, 1 (Cor. Lem. 3), we obtain

$$n y^{n-1} r + n a y^{n-2} r^2 + n b y^{n-3} r^3, \&c., n y r^{n-1} + r^n = z^n$$

or

$$(n y^{n-1} + n a y^{n-2} r + n b y^{n-3} r^2, \&c., n y r^{n-2} + r^{n-1})\, r = z^n.$$

And here it is evident, that the first side of the equation is divisible by r, *once*, and after that, neither by r, nor by any factor of r, except n be one of its factors, in which case the first side is divisible by $n\,r$, *once*, and after that, neither by n, nor r, nor by any factor of r; because the first term y^{n-1} has no common measure with r, but all the rest of the terms have; and, therefore, the whole quantity, taken collectively, has no common measure with r. And the same must necessarily be true of the equal quantity z^n, namely, that it is divisible by r, *once*, and after that, neither by r, nor by any factor of r, unless n be one of its factors, in which case it is divisible by $n\,r$, *once*, and after that, neither by n, nor r, nor by any factor of r; therefore r, in the first case, and $n\,r$, in the second case, must be a complete nth power (Lemma 2): but if $r\,n = \varphi^n$, n must be a factor of φ, that is $\varphi = n\,\varphi'$, therefore, $r\,n = n^n \varphi'^n$, or $r = n^{n-1}\varphi'^n$, that is $r = x - y$ must be of one of the former, φ^n, or $n^{n-1}\varphi'^n$.

And it is evident that we should have been led to the same result, if we had considered the equation under the form

$$x^n - z^n = y^n,$$

namely, that $x - z$ must also be of one of the forms φ^n or $n^{n-1}\varphi^n$.

If we investigate the same equation under the form $y^n + z^n = x^n$, and make $y + z = s$, or $s - y = z$, we shall find that s is prime to both y and z, for if s and y had a common measure, z must have the same, and if s and z had a common measure, then y must have the same, because $s - z = y$; since, therefore, y and z are prime to each other, s is also prime to both. Substituting therefore for z as above, our equation becomes

$$(s - y)^n + y^n = x^n$$

Or by expanding the binomial $(s - y)^n$ and substituting for the coefficients as before, we obtain

$$s^n - n s^{n-1} y + n a s^{n-2} y^2 - n b s^{n-3} y^3 + \&c.\ n s y^{n-1} = x^n$$

or

$$(s^{n-1} - n s^{n-2} y + n a s^{n-3} y^2 - n b s^{n-4} y^3 + \&c.\ n y^{n-1})\, s = x^n$$

And here, from the same reasoning as that employed above, we find that s must be of one of the forms ϕ^n, or $n^{n-1}\phi^n$.

Hence then if the equation

$$x^n - y^n = z^n$$

be possible, the following conditions must obtain, viz.

The difference of the roots $x-y$, of the form r^n or $n^{n-1} r^n$.

The difference of the roots $x-z$, of the form s^n or $n^{n-1} s^n$.

The ſum of the roots $y+z$, of the form t^n or $n^{n-1} t^n$.

But since $(x-y)$ or $(x-z)$ and $(y+z)$ are respectively divisors of the three nth powers, z^n, y^n, and x^n, and since these three quantities are prime to each other, their divisors must also be prime to each other, and consequently only one of these can be of the latter form above given, as they would otherwise have a common divisor n. Therefore, if the equation be possible, we shall have either

$$\begin{cases} x-y \text{ of the form } r^n \\ x-z \text{ ....} \ s^n \\ y+z \text{ ....} \ t^n \end{cases}$$

or, two of these quantities will be of this form, and the third of the form $n^{n-1}\phi^n$ which evidently resolves into the four following cases, one of which must necessarily obtain if the equation $x^n - y^n = z^n$ be possible, viz.

$$\text{1st, } \begin{cases} x-y = r^n \\ x-z = s^n \\ y+z = t^n \end{cases} \qquad \text{2nd. } \begin{cases} x-y = n^{n-1} r^n \\ x-z = s^n \\ y+z = t^n \end{cases}$$

$$\text{3d. } \begin{cases} x-y = r^n \\ x-z = n^{n-1} s^n \\ y+z = t^n \end{cases} \qquad \text{4th. } \begin{cases} x-y = r^n \\ x-z = s^n \\ y+z = n^{n-1} t^n \end{cases}$$

Q. E. D.

Proposition 2.

The equation $x^n - y^n = z^n$ is impossible in integers, n being any prime number greater than 2. Prop. 2.

We have before observed that x, y, and z, may be considered as prime to each other, and by the foregoing prop. it Demonstration.

is shown, that, if the equation be possible, one of the four following conditions must obtain, viz.

$$\text{1st}\begin{cases} x - y = r^n \\ x - z = s^n \\ y + z = t^n \end{cases} \qquad \text{2nd}\begin{cases} x - y = n^{n-1}r^n \\ x - z = s^n \\ y + z = t^n \end{cases}$$

$$\text{3d}\begin{cases} x - y = r^n \\ x - z = n^{n-1}s^n \\ y + z = t^n \end{cases} \qquad \text{4th}\begin{cases} x - y = r^n \\ x - z = s^n \\ y + z = n^{n-1}t^n \end{cases}$$

But at present we shall only consider one of those cases, for example the first, and in our result, by substituting $n^{n-1}r^n$ instead of r^n, $n^{n-1}s^n$ for s^n, &c., we shall arrive at all the possible cases. First then, let us ascertain whether the equation $x^n - y^n = z^n$ be possible, on the supposition that

$$x - y = r^n$$
$$x - z = s^n$$
$$y + z = t^n$$

Now from these three equations we derive the three following, viz.

$$x = \tfrac{1}{2}\left(t^n + (s^n + r^n)\right),$$
$$y = \tfrac{1}{2}\left(t^n + (s^n - r^n)\right),$$
$$z = \tfrac{1}{2}\left(t^n - (s^n - r^n)\right).$$

And consequently since $x^n - y^n = z^n$, or $x^n = y^n + z^n$, therefore

$$\left(\frac{t^n + (s^n + r^n)}{2}\right)^n = \left(\frac{t^n - (s^n - r^n)}{2}\right)^n + \left(\frac{t^n + (s^n - r^n)}{2}\right)^n,$$

or

$$(t^n + \overline{s^n + r^n})^n = (t^n - \overline{s^n - r^n})^n + (t^n + \overline{s^n - r^n})^n$$

now

$$(t^n + \overline{s^n - r^n})^n = t^{nn} + nt^{nn-n}(s^n - r^n) + nat^{nn-2n}(s^n - r^n)^2 + \&c.$$
$$(t^n - \overline{s^n - r^n})^n = t^{nn} - nt^{nn-n}(s^n - r^n) + nat^{nn-2n}(s^n - r^n)^2 - \&c.$$
$$(t^n + \overline{s^n + r^n})^n = t^{nn} + nt^{nn-n}(s^n + r^n) + nat^{nn-2n}(s^n + r^n)^2 + \&c.$$

And here, since the sum of the two first expressions is equal to the third, it is evident that the latter subtracted from the sum of the two former is equal to zero. But in adding the two first together, the 2nd, 4th, &c. terms cancel, and consequently in subtracting the latter from that sum, the 2d, 4th, &c. terms will remain the same, except that the signs will be changed from + to —. And as to the 1st, 3d, &c. terms of the first two equations, and the same terms of the third, we shall have (by observing that.

$(s^n - r^n)^2 = (s^n + r^n)^2 - 4 s^n r^n$
$(s^n - r^n)^4 = (s^n + r^n)^4 - 8 (s^{3n} r^n + s^n r^{3n})$
$(s^n - r^n)^6 = (s^n + r^n)^6 - 12 s^{5n} r^n - 40 s^{3n} r^{3n} - 12 s^n r^{5n}$
&c. &c.) for the sum of the two

1st terms $2 t^{nn}$
3d terms $2 n a t^{nn-2n} (s^n + r^n)^2 - 2 n a t^{nn-2n} \times 4 s^n r^n$
5th terms $2 n c t^{nn-4n} (s^n + r^n)^4 - 2 n c t^{nn-4n} \times 8 s^n r^n (s^{2n} + r^{2n})$
7th terms, &c.

And, consequently, subtracting from those sums, the 1st, 3d, &c. terms of the third line, namely,

1st term t^{nn}
3d term $n a t^{nn-2n} (s^n + r^n)^2$
5th term $n c t^{nn-4n} (s^n + r^n)^4$

the remainders of these particular terms will be

1st rem. $= t^{nn}$
3d rem. $= n a t^{nn-2n} (s^n + r^n)^2 - 2 n a t^{nn-2n} \times 4 s^n r^n$
5th rem. $= n c t^{nn-4n} (s^n + r^n)^4 - 2 n c t^{nn-4n} \times 8 s^n r^n (s^{2n} + r^{2n})$
7th rem. &c. &c.

In short, the whole of the remainder which is equal to zero, will be expressed by

$$(t^n - \overline{s^n + r^n})^n - (2 n a t^{nn-2n} \times 4 s^n r^n) - (2 n c t^{nn-4n} \times 8 s^n r^n) (s^{2n} + r^{2n}) - \&c.$$

And here it is only necessary to observe, that all the terms on the latter side of this expression are divisible by $t^n s^n r^n$, so that, for perspicuity sake, we may write it thus

$$(t^n - \overline{s^n + r^n})^n - t^n s^n r^n A = 0$$

and consequently

$$(t^n - \overline{s^n + r^n})^n = t^n s^n r^n A$$

and here, since the first side is a complete nth power, the latter side, which is equal to it, must be so likewise, and consequently A must be a complete nth power, or $A = A'^n$; that is

$$(t^n - \overline{s^n + r^n})^n = t^n s^n r^n A'^n$$

and, therefore,

$$t^n - s^n - r^n = t s r A'.$$

or, dividing by $t r s$, we have

$$\frac{t^{n-1}}{sr} - \frac{s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A',$$

which muſt necessarily be an integer. But these three fractions are each in their lowest terms, because r, s, and t, are prime to each other, and each of the denominators contains a factor that is not common to the other two; they cannot therefore be equal to an integer, by cor. Lemma 1, and consequently the equation is impossible under the first condition. And in order to arrive at the results of the other three conditions, we have only to substitute $n^{n-1} r^n$ instead of r^n, $n^{n-1} s^n$ for s^n, and $n^{n-1} t^n$ for t^n, whence we draw the following four conclusions,

$$1\text{st}\quad \frac{t^{n-1}}{rs} - \frac{s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A'$$

$$2\text{nd}\quad \frac{t^{n-1}}{rs} - \frac{s^{n-1}}{tr} - \frac{n^{n-1} r^{n-1}}{st} = A''$$

$$3\text{d}\quad \frac{t^{n-1}}{rs} - \frac{n^{n-1} s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A'''$$

$$4\text{th}\quad \frac{n^{n-1} t^{n-1}}{rs} - \frac{s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A''''$$

according as we assume the 1st, 2nd, 3d, or 4th, condition. And in each of these expressions we ought to have A', A'', A''', A'''', integer numbers, if the given equation were possible; but since in each of these expressions we have three fractions each in its lowest terms, and the denominator of each contains a factor not common to the other two, therefore by Lemma 1, and its corollary, they cannot produce an integer number.

Having shown, therefore, that, if the equation $x^n - y^n = z^n$ were possible, one of the quantities A', A'', A''', or A'''', would be an integer, and having also demonstrated, that no one of these quantities can be an integer, it follows that the equation whence they were derived is impossible; that is, the equation $x^n - y^n = z^n$ is impossible, when n is a prime number.

But we have also shown, that the impossibility of the equation $x^n - y^n = z^n$, when n is a prime, involves with it the impossibility of every equation of the form

$$x^n + y^n = z^n,$$

in which n is any number whatever except 2, or some power of 2; and we have likewise shown, that the impossibility of the equation, when n is any power of 2, is involved in that of $x^4 - y^4 = z^4$, which particular case has been proved to be impossible by Euler; and consequently the equation

$$x^n \pm y^n = z^n$$

is always impossible, when n is any integer number whatever greater than 2. And since the equation $x^n \pm y^n = z^n$, is impossible, so also is $\frac{x^n}{m^n} \pm \frac{y^n}{p^n} = \frac{z^n}{q^n}$, for this is the same as $x^n + y^n = \frac{z^n m^n p^n}{q^n}$: and therefore the equation is likewise impossible in fractions.

Q.E.D.

V.

Method of raising large Stones out of the Earth; by Mr. ROBERT RICHARDSON, *of Keswick, in Cumberland**.

GENTLEMEN,

I, Robert Richardson, of Keswick, in the parish of Crosthwaite, and county of Cumberland, beg leave to inform you, that I have found out a method of taking large self-stones out of the ground in a very expeditious manner; and that by this means two men will take as many stones out of the ground in one day, as would require twelve men in

New method of taking large detached stones out of the ground.

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* Trans. of the Soc. of Arts, vol. xxxi, p. 490. The silver medal was voted to Mr. Richardson for this invention.

the usual way of blasting, and afterward using large levers, &c.

Where stones from two to four tons each are to be taken up, two men will raise as many as twenty men in the usual way. The work is done by the power of a tackle, but by my method of fixing the tackle to the top of the stone, by the plug which I have invented, it will hold till the stone is pulled out of the ground, and laid upon the surface, or upon a carriage, if required, all which can be done in a very little time.

Stones of four tons weight, or upwards, may be taken out of the ground within the time of five or ten minutes, by two men, without any earth or soil being previously taken from around them, or without any digging with hacks or spades. J. C. Curwen, Esq., of Workington, has seen and approved of my performance with this invention, and if the Society should think it deserving of a premium, it would ever be gratefully acknowledged by

Gentlemen,

Your most obedient humble servant,

ROBERT RICHARDSON.

Keswick, Feb. 8, 1808.

DEAR SIR,

Efficacy of this method.

I cannot suffer Mr. Richardson's letter to be sent to the Society, without adding a few lines concerning it. I can bear ample testimony to the ease with which the largest self-stones are lifted by this method. I have seen one upwards of five tons lifted by four men. One of the plugs is sent for the inspection of the Society. There is no difficulty in cutting the hole to receive it, the only care is not to make it too large. It is difficult to explain the theory of its action; the least stroke laterally disengages the stone.

Its uses.

In many situations it is likely to be of great use, not only in drawing stones out of the ground, but in making weirs and embankments, where the stones are only to be lifted a moderate height.

One of my farmers in Westmoreland has made great use of one, and speaks of it in high terms. I have exhibited it to numbers of persons, who could not believe its power, till they saw it tried.

Mr. Richardson submits its examination to the Society, and I conceive it will be very useful and beneficial in cases of new enclosures of land. I do not think it would answer for soft stones, or safe to use for raising stones in building, it being so easily disengaged by any lateral blow. By adding wheels to the tackle machine, or having it upon a sledge, a great deal of time and trouble would be avoided. I purpose to employ this method next summer in making an embankment against the sea; the facility it will give in raising and removing large stones will expedite the work greatly. If any farther certificates of the performance of this plug be required by the Society, I will with pleasure transmit them to you. I will answer for its extracting any stone not exceeding five tons weight out of the ground, without any previous moving of the earth; and it is of importance to preserve large stones entire.

I am, with respect, dear Sir,
Your obedient humble Servant,
J. C. CURWEN.

Workington Hall, Feb. 19, 1808.

SIR,

Farther remarks on it.

I am favoured with your letter, desiring my opinion of the utility of the iron plug invented by Robert Richardson, of Keswick. That which I use is about six inches long, and one inch and a quarter in diameter; it requires a hole of its own size, only two inches deep; the plug is to be driven in a little short of the bottom, and will raise a stone of six or eight tons, with the assistance of three men, in the course of ten minutes after the hole is prepared; and I do not hesitate to say, that three men, thus furnished, will clear the ground of large stones in less time, and more effectually, than twelve men by any other method yet come to my knowledge. The plug should be made of good beaten iron. The simplicity and cheapness of the whole apparatus is a great object, as a good plug of the size I use will cost only two shillings and sixpence. I am fully of opinion, that by adding more and stronger ropes and pullies, work might be done by it to an amazing extent. I have reaped great advantage in my farm from the aid of the iron plug, and, in justice to the inventor, am happy in thus vouching for its extreme usefulness. Several of my respectable neighbours have experienced the aid and benefit of the above instrument, and will vouch, if required, for the truth of the above statement.

I am, Sir,
Your truly obedient Servant,
ROBERT WRIGHT.

Rose Gill Hall, near Shap, Westmoreland, May 9, 1808.

Reference to the Engraving of Mr. Richardson's Invention for raising large Stones out of the Earth. See Pl. VI, Figs. 4, 5, 6, *and* 7.

The method described.

Fig. 4, K, shows the upper part of a stone nearly buried in the earth, having a hole made in it three inches and a half deep, and one inch in diameter, by means of a miner's jumper; the cylindrical tail of the plug *a*, figs. 5, 6, and 7, which is of the same size, is driven fast into it, by means of a hammer applied upon the head of the plug at G. This plug, in its whole length, is nine inches, and has a hole made in its broad part H, through which the oval iron ring B passes easily, and on which the plug can move backwards and forwards, when the ring is hung upon the hook of the lower pulley block of the lifting tackle. C C C C represent the four legs or frame work of the quadrangle; D a fivefold tackle, with blocks ten inches in diameter; E a roller seven inches in diameter, turned by two long iron levers *b b*; the handle I is used as a safeguard, and to assist to regulate the power of the levers. In fig. 4, the plug A is shown fixed in the stone K, ready to draw it out of the ground, by means of the lifting tackle.

N.B. The hinder legs of the quadrangle are made to close in between the fore legs, for the convenience of carriage.

VI.

An Account of a new Method of increasing the charging Capacity of coated Electrical Jars, discovered by JOHN WINGFIELD, *Esq., of Shrewsbury*, and communicated to Mr.* JOHN CUTHBERTSON, *Philosophical Instrument Maker, of Poland Street, Soho; with Experiments proving the above, by Mr.* JOHN CUTHBERTSON.

N my treatise, entitled *Practical Electricity and Galvanism*, p. 103, I have said, that breathing into coated jars increased their charging capacity to such an astonishing degree, that their discharge would fuse four times the length of wire they could do in ordinary circumstances, which I proved by experiment, p. 178, 194.

Breathing into jars increases their capacity for a charge.

Since that publication large electrical batteries are become more general, and the number of jars increased, so that batteries containing thirty, sixty, and even a hundred and more jars are frequently met with. When so numerous, breathing into each jar is very disagreeable; and not only that, but, when the atmosphere is very dry, and when it is most wanted, it is even ineffectual; because the jars, which were first breathed into, lose that property which was produced in them by breathing, before the last can have obtained it; so that a variety of other means have been tried, such as wetting their insides with water, and putting wet sponges into them, and also greasing and oiling the uncoated part in the inside, all of which gave unsatisfactory results; till John Wingfield, Esq., communicated to me, that pasting paper on the inside and outside of coated jars prevented them from exploding to the outside coating, and that he believed their charging capacity was increased thereby.

This inconvenient in large batteries,

and moistening with water ineffectual.

Paper pasted inside and out.

I embraced the first opportunity to try the effect of this discovery with single jars.

Trials of this.

Exp. 1. I took a very thick jar (which had been used

Exp. 1

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* A gentleman who has lately very much distinguished himself, not only in the electrical science, but in all other branches of experimental philosophy.

to show the phenomena of voluntary explosions without breaking) twelve inches high, and the coating nine inches, containing in the whole 171 square inches. It was applied to the conductor of a plate electrical machine, and six turns of the plate caused a voluntary explosion. The state of the atmosphere not being very dry, it required eight and twelve turns to produce a second and third explosion. A fourth could not be produced; but, when cleaned and dried, as before, six turns caused a voluntary discharge.

Exp. 2.

Exp. 2. A slip of paper one inch broad was taken of sufficient length to fit the outside of the jar, when the two ends were pasted together. This was slipped on the outside to about one inch from the coating, the uncoated part being rubbed clean and dry, and applied to the machine, eleven turns of the plate produced a voluntary discharge to the outside coating.

Exp. 3.

Exp. 3. The paper ring was then slipped down to touch the coating, and then applied to the conductor, no voluntary discharge could be produced, and when discharged in the common way, its power did not seem to be increased.

Exp. 4.

Exp. 4. The common discharging electrometer (which is always fixed to the basement of my machine) was used, to try to what distance the discharge could be made to pass from the knob of the conductor to the ball of the electrometer, which was found to be one inch and $\frac{3}{8}$.

Exp. 5.

Exp. 5. A piece of iron wire $\frac{1}{100}$ part of an inch in diameter, and one inch in length, was hung to the electrometer, through which a second discharge was made to pass, and the colour of the wire was changed to a blue.

Exp. 6.

Exp. 6. The paper ring was then taken off, and I breathed into the jar twice; the discharge was then produced at the distance of two inches, and the wire was fused into balls.

Exp. 7.

Exp. 7. The jar was then rubbed clean and dry, and a piece of the same sort of wire, of the same length, was hung to the electrometer in the same manner as before, and it appeared, that the greatest charge it could take had not the least effect upon the wire. Thus it appears, that a paper ring so applied does not increase the charging capacity of jars in the same degree as breathing.

Exp. 8. *Exp.* 8. The jar was highly charged, and examined in the dark. The paper ring appeared luminous all round the uppermost edge.

Exp. 9. *Exp.* 9. The ring was then taken off, and pasted on in the inside close to the coating. Twenty-three turns caused a voluntary explosion, through the ring, to the outside coating.

Exp. 10. *Exp.* 10. A second ring, ¾ of an inch broad, was then pasted on close to the other. The same number of turns produced a voluntary explosion, and the paper was torn by the discharge; after which it was repaired, and left to dry.

Exp. 11. *Exp.* 11. When dry, no voluntary explosion could be obtained.

Exp. 12. *Exp.* 12. Its greatest power was then tried, and it was found exactly the same as in Exp. 6, when it was breathed into; it discharged at 2 inches distance, and the same length of wire was fused into balls.

Exp. 13. *Exp.* 13. A second jar was taken of a larger size, being 13 inches high, and its coating 7 inches, it contained about 190 square inches. After being rubbed clean and dry it was applied to the conductor of the machine, 12 turns of the plate produced a voluntary explosion to the outside coating.

Exp. 14. *Exp.* 14. A paper ring was put round the uncoated part on the outside, at about 1¼ inch distant from the coating. Eleven turns of the plate produced a voluntary explosion to the outside coating. The paper ring was then pushed down to the coating, after which no voluntary explosion to the coating could be obtained, but it discharged to the electrometer ball standing at the distance of 2 inches and ⅝ from the knob of the conductor.

Exp. 15. *Exp.* 15. The same sort of wire as used in Exp. 6, two inches long, was hung to the electrometer, and the discharge made it blue, with several bendings: a proof that it had been nearly redhot.

Exp. 16. *Exp.* 16. A ring of common writing paper, one inch broad, was pasted on the inside close to the coating; and when dry no voluntary explosion to the coating could be obtained, but it discharged itself to the electrometer ball standing at the distance of 2⅝, and the wire was fused into balls.

Exp. 17. *Exp.* 17. The paper rings were now taken off, and the uncoated part made clean and dry. Nineteen turns produced a discharge to the electrometer ball at the same distance, and the same length of wire was slightly blued.

Exp. 18. *Exp.* 18. The jar was then breathed into, and a discharge was produced at the same distance, but the wire was not fused.

Exp. 19. *Exp.* 19. The same jar was breathed into a second time; a discharge was caused at the same distance, and the wire was fused into balls exactly the same as when the paper rings were on.

Exp. 20. *Exp.* 20. A third jar was taken, 7 inches high and 4 inches diameter, having about 64 square inches coating. When rubbed clean and dry, and applied to the machine, two turns of the plate caused a voluntary discharge to the outside coating.

Exp. 21. *Exp.* 21. A paper ring was pasted on both sides, close to the coating, and one inch from the top. When applied to the machine, no voluntary explosion could be obtained, but the electric fluid was seen to run over the brim of the glass to the coating as quickly as the machine could give it. The exploding distance to the electrometer was seven eighths of an inch.

Exp. 22. *Exp.* 22. The paper rings were taken off, and others pasted on, to within one inch and a half from the top. No voluntary explosion to the coating could be obtained; but it exploded to the electrometer standing at one inch distance, and did not produce any effect on one inch of wire, which was hung to the electrometer.

Exp. 23. *Exp.* 23. Then paper rings of different breadths were tried, and it was found, that, when they were only half an inch higher than the coating, the jar received the greatest power, and its exploding distance to the electrometer was one inch and three eighths, which fused and dispersed in balls one inch of wire, of the same diameter as that used in Exp. 6, with the first jar.

Exp. 24. *Exp.* 24. The paper ring was scraped from off both sides, and the jar was carefully breathed into. Then six turns of the plate caused its longest discharge to the electrometer, which was at the distance of one inch and three eighths, and fused one inch of wire, but with less violence than in the last experiment.

Exp. 25. The outside paper ring was scraped off, and the jar still preserved the same charging capacity, as when both were on. Exp. 25.

The above experiments are sufficient to prove, that paper rings pasted on to electrical jars not only prevent them from exploding to the outside coating before they have received their highest charge, but that they likewise increase their charging capacity; and that one ring pasted on in the inside only is sufficient, if it is one inch broad; one half of the breadth must be pasted upon the coating, and the other upon the uncoated part. General conclusions.

Farther experiments and observations, setting forth the advantages that electricians may obtain from the above discovery, with an account of some experiments done with a view to prevent the jars from being perforated by high charges, without increasing their thickness, wherein I am in hopes I shall succeed, will be the subject of a future paper.

VII.

On the Combinations of Oxigen. By MARSHALL HALL, *Esq. F. R. M. S. E. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

THE utility and excellence of axioms in science are too well known to those, who are earnestly engaged in its prosecution, to require to be expatiated on. The few observations on the combinations of oxigen, which I take the liberty of transmitting to you, do not perhaps deserve the dignified name of axioms; but where coincidences are so general and striking, we are led, perhaps too soon indeed, to believe them universal. If however I shall point out what generally takes Axioms of great use in science.

takes place, the subject will not want importance and interest to those who are about to begin the elevated study of chemistry: to such it will at least afford a useful aid to the memory, much to be desired.

The first two of these propositions will be perceived to be very generally known: it is the third, which is the most extraordinary, and which renders the former of more importance.

Laws of the combinations of oxigen.

First then it is known, that substances containing oxigen will unite to *each other:* and 2dly, substances which do not contain this principle readily and *mutually* combine: but, 3dly, I believe no substance containing oxigen will combine with a substance, which does not contain oxigen.

Acids combine with oxides, but not with metals.

Alkalis do the same.

Metals separate from oxides as soon as formed.

I shall now endeavour to elucidate and support the preceding propositions; and no circumstance can do this more forcibly, than the universal chemical fact, that metals do not dissolve in *acids,* until they have, by some means, acquired oxigen; but the instant the oxidation is accomplished, the solution takes place. In the same manner the alkalis do not combine with the metals, but the metallic oxides and the alkalis do combine, *e. g.* the ammoniuret of copper, &c.; again, the metals during the process of oxidation immediately *separate* from the oxide when formed. But to reduce the subject into its simplest form, we may observe—

Classification of bodies that combine.

1. That the metals, metalloids, and simple combustible bodies, combine with each other. 2. The oxides of the metals, the alkalis, and the acids, formed of the combustible bodies with oxigen, mutually combine. 3. But no part of the first class will combine with any part of the second. 4. The first class have no affinity for water. 5. The second class, and most of the saline substances, do dissolve in water.

Objections answered.

Union of alkalis and metallic oxides with combustibles;

It will now be incumbent on me to obviate some objections, which may naturally oppose what I have said; and the most prominent is the union of the combustible bodies with the alkalis and metallic oxides. But this objection is readily answered; for those combustibles, which enter into such a combination, really contain oxigen; they are sulphur, phosphorus, and sulphuretted hidrogen. I think it sufficient to observe, that from the late experiments of Mr. Davy, the former two are found to be oxides; and hence their combinations with the alkalis do not at all invalidate the preceding doctrine, but tend to confirm it. The sulphuretted hidrogen too does most probably contain oxigen, as every body has suspected, from its properties as an acid, even that distinctive one of reddening vegetable blues. "Kirwan was unable to form it by melting sulphur in a "vessel containing hidrogen gas; and the Dutch chemists "were equally unsuccessful either with this method, or by "passing hidrogen gas through a tube containing liquid "sulphur*." From this then it appears, that the presence of atmospheric air is necessary to form the sulphuretted hidrogen, and hence we may infer with probability, that oxigen enters into the combination.

and of sulphur with metals.

If sulphur contain oxigen, then it will naturally be asked, why does it combine with the metals, which do not contain oxigen? To this I am ready to answer, that it is in some cases certain, in others more than probable, that the metal is oxidated during the operation; not indeed from the atmosphere, or other external sources, but from the sulphur itself; and this need not appear strange, for in the same manner the metals are oxidated by the acids, which afterward dissolve them: but this is not hypothesis, it is founded on the most unexceptionable experiments. Berthollet "formed "metallic sulphurets, performing the experiments in an "earthen retort, and after taking every precaution to avoid "any source of uncertainty, he obtained sulphuretted *hi-*"*drogen*; the metals he used were iron, copper, and mer-"cury, the last afforded the largest quantity†." Here then is unequivocally the decomposition of the sulphur; and the experiment is a more important argument, because it was made with a very different view from that with which I have applied it, *i. e.* to prove, beyond the possibility of mistake, that, during the formation of metallic sulphurets, hidrogen is liberated; and, as this is proved, it remains, that the combination in the retort will contain a larger proportion of the other principles of the sulphur, one of which is oxigen.

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* Murray.

† Murray.

And

Production of heat and light in the formation of sulphurets.

And here I cannot help observing, that a very beautiful application may be made of this circumstance, to explain the production of heat and light during the formation of these sulphurets, a difficulty hitherto inexplicable. It is well known, that oxigen in some of its combinations retains more of light and heat than in others, now here is the transition of oxigen, as contained in sulphur, to a metal; it is by no means improbable, that in the latter compound it may be disposed to retain less of the light and heat than in the former; if this be the case, the appearances during the operation no longer present any difficulty.

Union of alkalis with oil.

The only other circumstance, which occurs in objection to the above propositions, is the combination of alkali and oil; and this objection also is very readily satisfied. "When " the oil is separated by an acid from soap, it is affirmed, " that in several respects its properties are altered, and that " in particular it is soluble in alcohol. During its con" version into soap, therefore, it must not merely have " combined with the alkali, but have undergone some other " change *." But a short quotation from La Grange is still more to the purpose. " There is," he says, " an absorp" tion of oxigen during the process of saponification; that " is to say, the oil becomes concrete by absorbing oxigen."

Solution of sulphur and phosphorus with oil.

There is indeed still another compound, which may be urged as unfavourable to this doctrine, viz. the solution of sulphur and phosphorus in oil, but it is equally invalid with the former, and another quotation from Mr. Murray's excellent work will again calm the clamour of objection. It has been remarked, that sulphur and phosphorus contain oxigen; now this is the composition of oil in the words of Mr. Murray. Lavoisier hence inferred, " that oil contains " 79 parts of carbon united to 21 of hidrogen. These must " however be regarded as approximations only; and *oxigen* " is probably also a constituent part of oil. That it is so " appears to be *established* by the decomposition of oil in " close vessels; when transmitted through an ignited tube, " carbonic acid and water are part of the products."

It is however evident, that these observations are intended to apply in cases more simple than to vegetable compounds, in which indeed the elementary ingredients generally acquire a perfectly different arrangement, in every case of combination with other substances.

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* Murray.

Oxigen essential to acrimony.

I shall just add a remark, which I have often made, that substances not containing oxigen are perfectly *mild* in their nature, and only acquire acrid or virulent properties when oxigen is added to them. Thus oxigen occasions the acidity, the alkaline property, and, as has been remarked to me, the causticity of other substances. To this ingredient all mineral remedies owe their powers, to this the metallic and some other poisons owe their deleterious properties.

If the above observations deserve to occupy a page or two in the valuable Philosophical Journal, their insertion will much oblige, Sir,

Your obedient servant,

MARSHALL HALL,
Member of the Royal Medical Society.

University of Edinburgh,
Sept. 21, 1810.

VIII.

On the Migration of Swallows. By THOMAS FORSTER, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

Remarks on the migration of swallows.

IN a former number of your Journal you were so good as to insert my table of the times of appearance and departure of several of our migratory birds; if you should think the ensuing observations on the swallow tribe worthy of notice and insertion, they are very much at your service.

Swift.

The swift, *hirundo apus,* which abounds in the neighbourhood of Hackney, and annually builds in great numbers in the old steeple, was not seen at Hackney after August the 13th; and several days previous to this its numbers were greatly decreased. On the 17th I happened to be at Ely, when I saw great numbers of these birds flying round the tower of the cathedral; since which I have not seen a single bird.

Holes of the bank martin occupied by toads in winter.

In consequence of the controversy so long carried on among naturalists, whether the swallow was a bird of paſsage, or whether it remained dormant during winter, I opened on the 12th instant several of the holes of *sand swallows (hirundines ripariæ)*, but found nothing in them except *toads* which had taken up their winter's lodging there.

The migration of swallows depends more on the wind, than on the weather.

There are many circumstances, which tend greatly to establish the opinion, that these birds migrate; for instance, they do not appear in spring and depart in autumn sooner or later according to the forwardness or backwardness of the season, but generally according to the direction of the prevailing current of air. The great prevalence of N. E. winds lately has occasioned a very great diminution of the numbers of swallows and martins this year much before their usual time; whence may we not infer, that they had taken their flight in a south western direction? for winged insects (the food of this tribe) are still very abundant, notwithstanding the cold winds.

Inquiry after facts respecting their torpidity.

If any of your numerous readers know of any well authenticated accounts of swallows having been found in a torpid state during the winter season, and will have the goodness to communicate the same through the medium of your Journal, I shall be much obliged to them, as I am collecting facts of this kind.

I remain, Sir,

Your constant reader,

THOMAS FORSTER.

London, Oct. 15, 1810.

IX.

III. *The Case of a Man, who died in consequence of the Bite of a Rattlesnake; with an Account of the Effects produced by the Poison.* *By* EVERARD HOME, *Eſq. F. R. S.*[*]

Well described cases of fatal bites of snakes very rare.

OPPORTUNITIES of tracing the symptoms produced by the bite of poisonous snakes, and ascertaining the local effects on the human body when the bite proves fatal, are of such rare occurrence, that no well described case of this kind is to be met with in any of the records that I have examined. I am therefore induced to lay before this Society the following account, with the view of elucidating this subject, in which the interests of humanity are so deeply concerned.

Man bitten by a rattlesnake.

Thomas Soper, 26 years of age, of a spare habit, on the 17th of October 1809, went into the room in which two healthy rattlesnakes, brought from America in the preceding summer, were exhibited. He teized one of them with the end of a foot rule, but could not induce the snake to bite it, and on the rule dropping out of his hand, he opened the door of the cage to take it out; the snake immediately darted at the hand, and bit it twice in succession, making two wounds on the back part of the first phalanx of the thumb, and two on the side of the second joint of the fore finger. The snake is between 4 and 5 feet long, and when much irritated bites the object twice, which I believe snakes do not usually do.

The snake bit twice.

Effects of the bite.

The bite took place at half past two o'clock. He went immediately to Mr. Hanbury, a chemist in the neighbourhood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. Hanbury considered him to be in a state of intoxication, and gave him a dose of jalap to take off the effects of the liquor, and made some slight application to the bites. It appeared on inquiry, that the man had been drinking; but that, before he was bitten, there was nothing

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[*] Abridged from the Phil. Trans. for 1810, p. 75.

unusual

Effects of the bite.

unusual in his behaviour. After leaving Mr. Hanbury the hand began to swell, which alarmed him, and he went to St. George's hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the fore-arm before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock the swelling extended to the elbow, and at half past four it had reached half way up the arm, and the pain had extended to the axilla. At this time Mr. Brodie, who visited him in my absence, first saw him; he found the skin cold; the man's answers were incoherent; his pulse beat 100 strokes in a minute, and he complained of sickness.

At 9 o'clock he had the feeling of great depression, his skin was cold, and his pulse weak, beating 80 strokes in a minute. At a quarter after 10 the pain had become very violent in the arm: his pulse was stronger, but fits of faintness attacked him every fifteen minutes, in which the pulse was not perceptible; yet in the interval his spirits were less depressed. At half after 11 the hand and arm were much swelled, up to the top of the shoulder, and into the armpit. The arm was quite cold, and no pulse could be felt, even in the armpit, where the swelling was such as to prevent the artery from being accurately distinguished. The wounds made on the thumb were just perceptible; those on the finger were very distinct. His skin in general was unusually cold.

In the morning of the 18th his pulse beat 132 strokes in a minute, and was very feeble. The swelling had not extended upward to the neck, but there was a fulness down the side, and blood was extravasated under the skin as low as the loins, giving the back on the right side a mottled appearance. The whole of the arm and hand was cold, but painful when pressed; the skin was very tense; on the inside of the arm vesications had formed below the armpit and near the elbow, and under each of the blisters was a red spot of the size of a crown. The skin generally over the body had become warm. At noon the skin of the whole arm had a livid appearance, similar to what is met with in a dead body, when putrefaction has begun to take place, unlike

Effects of the bite.

unlike any thing I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist and forearm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged.

On the 19th his pulse was scarcely perceptible: his extremities were cold: the vesications were larger, the size of the arm was diminished; and he had sensation in it down to his fingers.

On the 21st the size of the arm was farther reduced, but the skin was extremely tender.

On the 22d the right side of the back, down to the loins, was inflamed and painful; and had a very mottled appearance from the extravasated blood under the skin.

On the 23d the vesications had burst. On the 26th the arm was more swelled and inflamed. The inflammation increased; and on the 28th a slough had begun to separate from the inside of the arm below the armpit. On the 29th a large abscess had formed on the outside of the elbow, which was opened, and half a pint of reddish brown matter was discharged with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller; but the upper part continued tense.

On the 30th the redness and swelling of the upper part of the arm had subsided. On the 31st the discharge from the abscess had diminished. On the 1st of November ulceration had taken place on the opening of the abscess, so that it was much increased in size. The next day this ulceration had spread to the extent of two or three inches; and mortification had come on in the skin nearer the armpit. On the 3d the mortification had spread considerably; and the fore finger, which had mortified, was removed at the second joint. And on the 4th of November he died at half after four o'clock in the afternoon.

Appearances on dissection.

Sixteen hours after death, the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the hospital.

With the exception of the right arm, which had been bitten, Appearances on dissection.

bitten, the body had the natural appearance. The skin was clear and white; and the muscles contracted.

The wounds made by the fangs at the base of the thumb were healed, but the puncture made by the lancet at the back of the wrist was still open. That part of the back of the hand, which immediately surrounded the wounds made by the fangs, for the extent of 1½ inch in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside of the arm, elbow, and forearm, near six inches in length. Around this, the skin was in a state of mortification, more than half way up the outside of the arm, and as far downwards, on the outside of the forearm. The skin still adhered to the biceps flexor muscle in the arm, and flexor muscles in the forearm, by a dark coloured cellular membrane. Every where else in the arm and forearm, from the axilla downward, the skin was separated from the muscles, and between these parts there was a dark coloured fluid, with an offensive smell, and sloughs of cellular membrane resembling wet tow, floating in it. The muscles had their natural appearance every where, except on the surface, which was next the abscess. Beyond the limits of the abscess, blood was extravasated in the cellular membrane; and this appearance was observable on the right side of the back as far as the loins, and on the right side of the chest over the serratus major anticus muscle.

In the thorax the lungs had their natural appearance. The exterior part of the loose fold of the pericardium, where it is exposed on elevating the sternum, was dry, resembling a dried bladder. The cavity of the pericardium contained half an ounce of serous fluid, which had a frothy appearance, from an admixture of bubbles of air. On cutting into the aorta, a small quantity of blood escaped, which had a similar appearance. The cavities of the heart contained coagulated blood.

In the abdomen, the cardiac portion of the stomach was moderately distended with fluid: the pyloric portion was

Appearances on dissection.

much contracted; the internal membrane had its vessels very turgid with blood. The intestines and liver had a healthy appearance. The gall bladder was moderately full of healthy bile. The lacteals and the thoracic duct were empty; they had a natural appearance.

In the cranium the vessels of the pia mater and brain were turgid with blood; the ventricles contained rather more water than is usual, and water was effused into the cells, connecting the pia mater and tunica arachnoides. It is to be observed, that these appearances in the brain and its membranes are very frequently found in cases of acute diseases, which terminate fatally.

Mr. Home then adds two cases, that were sent from India to the late Dr. Patrick Russell, which correspond in many of the circumstances with the preceding; and an experiment he made formerly in the island of St. Lucia on the effects of the poison of a snake on two rats. The first case is that of a boy, who was bitten by a snake, called *kamnlee* by the natives, in the lower part of the arm, at 8 o'clock in the evening. The blood flowed very freely for some time. He died next day at noon in great pain.

The second is that of a sepoy, 60 years of age, bitten on the back part of the hand by a *cobra di capello.* He recovered, though slowly.

The paper concludes with the following observations.

Effects of the bites of snakes different.

It appears from the facts, which have been stated, that the effects of the bite of a snake vary according to the intensity of the poison.

When the poison is very active.

When the poison is very active, the local irritation is so sudden and so violent, and its effects on the general system are so great, that death soon takes place. When the body is afterward inspected, the only alteration of structure met with is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed.

When less &c.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe: in about half an hour, swelling takes place from an effusion of serum in the cellular membrane, which continues to increase with greater or less rapidity for about twelve hours, extending during that period into the neighbourhood of the bite; the blood ceases to flow in the smaller vessels of the swoln parts; the skin over them becomes quite cold, the action of the heart is so weak, that the pulse is scarcely perceptible, and the stomach is so irritable, that nothing is retained in it. In about 60 hours these symptoms go off, inflammation and suppuration take place in the injured parts, and when the abscess formed is very great, it proves fatal. When the bite has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

Patients who recover.

In those patients, who recover with difficulty from the bite, the symptoms produced by it go off more readily, and more completely, than those produced by a morbid poison, which has been received into the system.

Supposed efficacy of medicines.

The violent effects which the poison produces on the part bitten, and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief, that the recovery depended on the medicines employed; and in the East Indies eau de luce is considered as a specific for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for when the poison is so intense, as to give a sufficient shock to the constitution, death immediately takes place; and where the poison produces a local injury of sufficient extent, the patient also dies, while all slighter cases recover.

The effect of the poison on the constitution is so immediate, and the irritability of the stomach is so great, that there is no opportunity of exhibiting medicines, till it has fairly taken place, and then there is little chance of beneficial effects being produced.

Treatment.

The only rational local treatment to prevent the secondary mischief is making ligatures above the tumefied part, to compress the cellular membrane, and set bounds to the swelling, which only spreads in the loose parts under the skin; and scarifying freely the parts already swoln, that the effused serum may escape, and the matter be discharged, as soon as it is formed. Ligatures are employed in America, but with a different view, namely, to prevent the poison being absorbed into the system.

X.

Analyses of various Minerals, by Mr. Klaproth.

(Concluded from p. 155.)

Analyses of talc and mica.

Talc and mica require to be discriminated.

AMONG the minerals which are most commonly known there are several, the analysis of which deserves to be repeated, for the purpose of correcting their classification. Though talc and mica may be distinguished from each other in strongly marked specimens, which serve as types of the two species, they have a great deal of similitude in their external characters. But as nature is far from having separated minerals by limits as well marked, as those we are obliged to employ in our systems, in order to facilitate a knowledge of them, there are found between mica on the one hand, which belongs to the argillaceous genus, and talc on the other which belongs to the magnesian, a great many minerals, occupying various places between the archetypes of the two species, and perplexing the mineralogist in his determination where to place them.

Thus Mr. Haüy has classed among the talcs several minerals, which he is apprehensive will not ultimately be allowed to retain their place. " I confess," he says, " that among the minerals I have included under the name of talc, there are perhaps several, which chemical analysis will not suffer to remain there: but it appears to me at present premature, to make any change in this part of the system; particularly as I find we have no analysis of pure talc, that is not very old. This, therefore, should be reexamined, that we may have a precise knowledge of the substance taken as the standard of comparison."

As it is the same with mica, Mr. Klaproth undertook a comparative analysis of these two minerals, with a view to assist those naturalists, who do not confine themselves to external characters in their classification of substances, but pay attention likewise to their chemical characters.

1. *Lamellar talc of St. Gothard.*

Lamellar talc of St. Gothard

It was proper for this analysis to choose a talc, that should answer very strictly to all the mineralogical characters of the species, and accordingly that of St. Gothard appeared well suited to the purpose.

described.

Its colour is a silvery white, in some parts verging to an apple-green: in mass it is very brilliant, with a pearly lustre: its lamellar fracture is wavy: it is translucid, and the thin laminæ are transparent: it is tender, soft, flexible without being elastic, greasy to the touch, and moderately heavy.

Action of heat on it.

By calcination this talc lost half a part per cent, but no other remarkable change took place in it.

Exposed to the heat of a porcelain furnace, in a charcoal crucible, it was hardened, fell to pieces like a schist, acquired a whitish gray colour, and was fused in some places. Exposed to the same heat in a clay crucible, the result was the same, except that the colour had become a yellowish white.

The results of its analysis were

Component parts.

Silex	62
Magnesia	30·5
Oxide of iron	2·5
Potash	2·75
Loss by calcination	0·5
	98·25.

No chrome in green talcs.

Though the talcs that have a greenish colour are said to contain chrome, Mr. Klaproth could not find any perceptible trace of it.

Mr.

Analysis by Vauquelin.

Mr. Vauquelin has published in the Journal des Mines, No. 88, an analysis of a flexible lamellar talc, of a silvery white when in thin scales, in which he found

Silex	62
Magnesia	27
Oxide of iron	3·5
Alumine	1·5
Water	6
	100.

In regard to the principal parts, the silex and magnesia, these analyses pretty nearly agree: but they differ in Mr. Klaproth finding a much less loss by calcination, and no trace of alumine; while on the other hand Mr. Vauquelin says nothing of potash.

2. *Common mica of Zinnwalde.*

Magnesia not essential to flexible stones.

If mica were not formerly distinguished from talc in a proper manner, it was partly owing to the opinion given by the celebrated Dr. Black, in his Elements of Chemistry, that the earth of talc, or magnesia, was always one of the component parts of flexible stones. The old analyses of mica tended to perpetuate this errour, as it was always said, that mica contained magnesia, and belonged to the magnesian genus; and Kirwan, in his Mineralogy, speaks of having found 20 parts of magnesia in 100 of colourless mica.

Chenevix.

Haüy.

Mr. Chenevix even goes so far as to say, that talc and mica scarcely differ, and that he has found in them the same component parts in similar proportions: see Ann. de Chim. XXVIII, 200. And Mr. Haüy expresses himself as follows on the uncertainty between the limits of these two kinds of stone. "The name of talc, like that of spar, has been given to a number of minerals very different in their nature. It has been applied generally to a mineral capable of being divided into thin laminæ parallel to one of its faces, as is the case with mica, Venetian talc, sulphate of lime, &c. With respect to the species in question, The name of talc was employed in contradistinction to that of mica; talc signifying a mica in large laminæ, and mica talc in small scales. It was supposed to have been observed too, that talc was softer to the feel, and mica more harsh; but the point of separation, where talc ceases to be mica, and mica talc, still remained to be determined."

To fix the boundary between these two minerals with accuracy, a strict investigation of the component parts of mica was still requisite. Accordingly Mr. Klaproth took the common mica of Zinnwalde, in the mountains of Bohemia, for this purpose. It is found over a tin mine; is of a silvery white mingled with gray; and is crystallized in hexagonical laminæ, a little elongated, of an elastic flexibility, and usually arranged in the form of a rose.

Common mica from Bohemia.

While this elastic kind of flexibility may serve as one of the external characters to distinguish mica from talc, which has only an ordinary flexibility, the manner in which mica comports itself on exposure to heat is still more characteristic. When mica is heated redhot, its silvery white is changed to a deeper gray, but no diminution of weight is observed. Before the blowpipe on a piece of charcoal it melts readily into a shining, rounded bead, of a grayish black. Exposed to the heat of a porcelain furnace in a clay crucible, it fuses into a dark opake glass; and in a charcoal crucible into a semitransparent glass, covered with grains of iron.

Action of heat.

The results of its analysis were

Component parts.

Silex	47
Alumine	20
Oxide of iron	15·5
——— manganese	1·75
Potash	14·5
	98·75.

3. *Siberian mica in large laminæ.*

Muscovy glass.

The mica in large laminæ, or Muscovy glass, called in Russia *slinda*, differs so much from common mica in the largeness of its laminæ, and in its splitting perfectly straight, that a chemical analysis was requisite to determine whether it be in reality a variety of mica. It is chiefly used in Russia and Siberia instead of glass for windows, on which account it is an article of trade. It is found in the remotest parts of Siberia, on the other side of the river Lena, and almost always near the rivers Vitim and Mama. It occurs in a coarse-grained granite, and in large masses of quartz, either in nodules of different sizes, or in thick laminæ lying in various directions. It is got out with the mallet and chisel. As the rock is very hard, it is wrought only to the depth of a fathom by Russian colonists, who form companies for this purpose, and go and reside in the neighbouring woods during the summer. After the mica is got out of the quarry, it is sorted according to the size and clearness of the plates, and then carried to market, chiefly at Irkutzk, whence it is sent to various places.

The pieces fresh taken from the quarry, and not split, have a smoky or brownish colour, are opake, and reflect objects like a mirror; but in thin plates the colour disappears, and the mica is transparent. Their price varies considerably, according to the size of the plates: some are 36 or 40 inches square; but in general they are only three or four inches, and such as are less than this are worth very little.

Action of heat

Before the blowpipe, on charcoal, Muscovy glass loses its transparency, and becomes of a silvery white, but does not melt like common mica. If large plates of mica be heated redhot in crucibles, or on charcoal, they acquire a striking appearance of thin laminæ of silver, and experience a loss of 1·25 per cent. This mica is infusible even by the heat of a porcelain furnace. In a charcoal crucible, in which several of these laminæ, rolled one upon another, had been placed, the outer ones were found of a gray colour, glazed, and fragile; the inner ones were as black as tinder, and flexible. In a clay crucible all were hardened, vitrified, fragile, and sonorous; and their colour was of a grayish white, the surface only being in part light brown.

The results of its analysis were

Component parts.

Silex	48
Alumine	34·25
Oxide of iron	4·5
Magnesia, mixed with a little oxide of manganese	0·5
Potash	8·75
Loss by calcination	1·25
	97·25.

4. *Black Siberian mica.*

Black Muscovy glass.

Another variety, found in similar situations with the preceding, is the black mica, or black Muscovy glass. This differs both from the preceding and the common mica, not only in appearance, but in the proportions of its component parts. The following is Karsten's description of it.

When in large masses it appears black, but in thin plates it is a deep olive green. Before the laminæ are separated, they exhibit metallic reflections of green, blue, and red, on being held under different angles to the light. It may be obtained in large plates, and these split into thinner, which, by their tendency to form rhombs, indicate a secondary juncture. The principal fracture is lamellar, with very shining laminæ of a greasy and semimetallic lustre. This mineral is very tender, extremely smooth, and perfectly transparent when the laminæ are very thin, though entire pieces are opake. The laminæ have a perfectly elastic flexibility.

This substance is employed scarcely for any thing but lining little boxes either of wood or pasteboard.

Action of heat.

Before the blowpipe on charcoal it does not appear to undergo fusion except at the edges of the laminæ. If larger plates be heated redhot in a crucible, they acquire a tombac brown colour with a metallic brilliancy. The leaves split, and appear friable. They lose one per cent.

The results of the analysis are

Component parts.

Silex	42·5
Alumine	11·5
Magnesia	9
Oxide of iron	22
——— manganese	2
Potash	10
Loss by calcination	1
	98.

From what has been said we may conclude:

Difference between talc and mica.

1. That pure talc contains magnesia, and no alumine, which is a decided characteristic of this stone.

2. That common mica contains alumine, and no magnesia.

3. That Muscovy glass differs from common mica by its refractoriness, its larger proportion of alumine, its smaller proportion of oxide of iron, and its containing a trace of magnesia.

4. That the black mica of Siberia deserves to be considered as a variety differing both from common mica and Muscovy glass by its proportions of alumine and magnesia, as well as by its larger proportion of oxide of iron.

5. That mica, and its different varieties, are to be reckoned among the richest of the potassiferous minerals.

XI.

Description of the Dichroit, a new Species of Mineral: by Mr. L. CORDIER, *Mine Engineer in Chief*[*].

Mineral possessing a new property.

THE mineral I am about to describe belongs to the class of earthy substances. Its proper place in a system appears to be next the emerald; and it would not be more remarkable than most species of the same class, if it were not endued with a particular property, the knowledge of which may be interesting perhaps to those philosophers, who study the course of light through crystallized mediums.

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[*] Abridged from the Journal des Mines, vol. XXV, p. 129.

Where found.

This mineral was found at Cape de Gattes, in Spain. It was already known to the inhabitants of the country, and the lapidaries of Carthagena, when Mr. Launoi, a dealer in minerals, visited the place about twenty years ago, and brought away some specimens, which have been sold, part in France, and part in Germany. Most of these specimens being badly defined, they added to collections a rarity, of which science took no account.

Not yet analysed.

Being at Cape de Gattes a few years ago, I was fortunate enough to meet with some pieces of the mineral in question, all the essential characters of which were sufficiently decided, and indicated a new species. I purposed to give a description of it, as soon as I had analysed it; but not yet having had an opportunity of doing this, I am induced to publish my mineralogical observations on it, particularly as some foreign mineralogists have been beforehand with me.

Mentioned by some German writers.

Mr. Reuss, in the last volume of his treatise published in 1806, announces, that Werner has made a new species of the substance from Cape de Gattes, by the name of *yolith*; that he has ranged it next the cat's-eye, and divided it into three varieties, the vitreous, porphyritic, and common. Mr. Karsten, adopting Werner's opinion, in his Mineralogical Tables for 1808, has placed the yolith between the lazulite and andalousite of Delamétherie, and gives the following description of it.

Karsten's description.

"This mineral is found of a deep lavender blue, in mass or disseminated; of a feeble lustre, verging from brilliant to shining; with an uneven fracture, the fragments of which are indeterminate, and with very acute edges; the separate pieces are indistinct, and large grained. It is hard, brittle, opake, and moderately heavy. It is found at Cape de Gattes in Spain, associated with lithomarge, quartz, and crystallized almandine."

It is difficult to find in this description the characters, that induced Werner and Karsten to make a particular species of the mineral in question, for it is equally applicable to varieties of known substances, and particularly to the blue tourmaline.

Before I proceed to describe the dichroït, I ought to observe, that it is not mentioned in Delamétherle's Theory of the Earth, Haüy's Treatise on Mineralogy, Patrin's work, Brongniart's, the Abstract of Haüy's Method by Lucas, or any other work yet published in France.

How found.

Hitherto the dichroït has been found only in amorphous or crystallized grains, sometimes collected in small masses, not four inches in diameter.

Essential character.

Its essential character is its being divisible parallel to the faces of a regular hexaedral prism, and capable of subdivision by longitudinal sections perpendicular to the lateral faces.

Physical characters.

Its specific gravity is 2·56.

It scratches glass strongly, quartz feebly; and is easily broken.

Its fracture is vitreous, tolerably shining, and frequently giving very evident indications of scales.

Its fragments are irregular with sharp edges.

Its powder feels very rough.

The lustre of the external surface commonly dull.

The translucid crystals exhibit a particular phenomenon, which may be called that of double colour by refraction.

Geometrical characters.

Its primitive form is a regular hexaedral prism.

Its integrant particle is a triangular prism, the bases of which are scalene rectangles.

Chemical characters.

It is not acted upon by acids.

Before the blowpipe it fuses into a very light greenish gray enamel. A similar result is obtained either with borax or carbonate of soda.

Distinguishing characters.

The dichroit is distinguishable, 1, from the emerald, because the specific gravity of the latter is greater in the proportion of 10 to 9; its integrant particle is an equilateral triangular prism; and it fuses more difficultly: 2, from the tourmaline, in not becoming electrical by heat, and in being less hard, and less heavy: 3, from the corundum, in the latter being infusible, and affecting a rhomboidal primitive form: 4, from the dipyre, or leucolite, because the latter fuses with ebullition, and its powder is more phosphoescent: 5, from the nepheline, or sommite, because pieces of the latter immersed in nitric acid become cloudy internally, and its specific gravity is less in the proportion of four to five: 6, from the haüyne, in the property of the latter to resolve into a jelly in acids.

Varieties.

There are four varieties. 1. The primitive dichroit, which is a regular hexaedral prism.

2. The peridodecaedral. A rectangled prism, with twelve faces inclined to each other at angles of 150°.

3. Amorphous. In large irregular grains, exhibiting the rudiments of crystallization.

4. Granular. In irregular masses, formed of very large grains confusedly aggregated.

Transparency.

With respect to transparency, it is sometimes translucid, sometimes opake.

Colour.

All the crystals, or grains, viewed by reflected light, are of a violet colour, which is generally less bright in the longitudinal direction of the prisms.

Double refraction.

All the translucid crystals or grains, seen by refracted light, are both of a brownish yellow and an indigo blue. When viewed parallel to the axis of the prism, they constantly exhibit a very deep blue: but when viewed perpendicularly to this axis, they are of a very light brownish yellow. In the second case the transparency appears to be increased in the proportion of six to one.

Where found.

The dichroït is found in two places at Cape de Gattes; namely, at Granatillo, near Nijar, where its situation was verified anew last year by Mr. Tondi; and at the foot of the mountains surrounding the bay of San Pedro. The preceding description is drawn up from specimens from the latter place. They are found there in a vast horizontal ledge of volcanic breccia. This breccia is composed of detritus of every kind, but more particularly of fragments and blocks of black or red scoriæ in perfect preservation, of black vitreous lava, and of lithoid lava, either basaltic or petrosiliceous. The dichroit is found chiefly in blocks of the latter kind. Sometimes it occurs in the form of scattered grains, sometimes of crystals grouped, and as it were imbedded in the lava. It is found also not only in the gray or whitish tufa, which serves as a base to the breccia, but also in some of the fragments of foliaceous granite, which it includes. These fragments have evidently been exposed to the action of heat; and the primitive stratum, from which they have been detached, is very probably the original matrix of the dichroït. In fact they exhibit in their composition scales of black mica, and trapezoidal red garnets similar to those we see contained in the masses, and even in the interior of the crystals of this mineral, which indicates a contemporaneous formation. The petrosiliceous lava, that commonly serves as a gangue, is rather granular than compact. It is of the same nature as that of the Ponce Islands, or that of the Puy-de-Dôme, and of the cascade of Mont-d'Or in France, being composed of very fine grains of feldspar. The fire has left some very evident traces of its action on the crystals and masses of dichroït: most of the masses appear as if corroded in different places, both internally and externally, and in the cavities are seen portions of white scoriæ, either intact or decomposed. The crystals are almost all partly fused, cracked, and full of flaws. Their fragments frequently present surfaces rendered dull by an extremely thin whitish coating, that conceals the lustre of the fracture.

Reasons for making it a new species,

and giving it the name of dichroit.

From what has been said it appears, that the mineral of Cape de Gattes differs from all other known substances. Its primitive form, specific gravity, property of transmitting rays of two different colours, and the other positive or negative characters, that distinguish it more particularly from each of the substances, with which it is most likely to be compared, are so discriminative, that we cannot avoid considering it decidedly as a new species, without recurring to the testimony of chemical analysis. At the same time it seems to me most suitable to give it a name from its remarkable property of double colour, and such is the etymology of the name, which Mr. Haüy had the goodness to suggest to me. I conceive myself sufficiently authorized to reject the denomination of yolite (violet stone), derived from the superficial colour of the crystals, because in the present case its application would be more inconsistent than in many others. Besides, it is too liable to be confounded with hyalite, appropriated to the concrete hyaline quartz, or yanolite, or yonolite, given by Delamétherie to the old violet schoerl.

It probably produces a double image.

From the primitive form of the dichroit it is to be presumed, that it possesses the property of producing a double image; but this I could not ascertain, for want of crystals sufficiently transparent. The verification of this conjecture however would be the more interesting, as the phenomenon of double refraction could take place only in a direction oblique to the axis of the prism; of which I have satisfied myself by experiment, and which the phenomena of the emerald sufficiently confirm. Hence we see, that, on the hypothesis of a double refraction, there would be such a relation between this phenomenon and that of the double colour, that the crystals would double images in the direction in which the colours appear mingled, while we should see a simple image by looking in that direction, in which each colour becomes exclusive.

XII.

Analysis of the Nadelertz of Siberia: by Mr. JOHN[*].

Nadelertz an ore of bismuth.

THE needle-ore has been considered in Russia as an auriferous ore of nickel. In the work of Reuss, and in the Ephemerides of baron Moll, it is classed among the ores of chrome: but the analysis of Mr. John, given in Gehlen's Journal, shows, that it is an ore of bismuth.

The following are its external characters according to Karsten.

Its external characters.

Its colour is steel gray, sometimes a pale copper red, or covered with a green and yellow coating[†].

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[*] Journal des Mines, vol. xxiv, p. 227.

[†] The yellow coating is so slight, Mr John could only examine it by wiping it off with cotton moistened with nitric acid, washing the cotton in water, and evaporating the water and excess of acid It then appeared to him, from such experiments as he could make, to be oxide of uranium. The green coating, covering both the crystals and the quartz gangue, is thicker and more abundant. From his analysis it consisted of carbonate of copper, carbonate of lead and bismuth.

Colour, where scraped, scarcely deeper than that of the fresh and shining ore.

It is disseminated, and crystallized in sixsided elongated prisms, accumulated in the form of needles. These crystals are sometimes curved, or jointed, always imbedded, and frequently crossing each other.

Their surface is striated longitudinally.

They have seldom any perceptible lustre on account of the coating. When this is wanting, their external lustre is but little. Interiorly it is always metallic.

Their fracture lengthwise is foliated, and very brilliant: transverse, uneven and brilliant.

Fragments, unknown.

Opake.

Feels smooth.

Soft.

Extremely heavy; its specific gravity being 6·125.

It is found in the mines of Pischminskoi and Klintzefskoi, near Ekaterinenbourg, in Siberia.

Its component parts, supposing the gold and quartz to be accidental mixtures, are

Component parts.

Bismuth	43·20
Lead	24·32
Copper	12·10
Nickel?	1·58
Tellurium?	1·32
Sulphur	11·58
Loss (oxigenized sulphur?)	5·90
	100·00

Patrin had considered it as an ore of bismuth long ago.

In a note subjoined to the preceding paper Mr. Patrin observes, that when he was at those mines in 1786 the needle-ore had just been discovered; and as its nature was not known to the managers of it, they made a secret of the particular spot where it was found. With some difficulty however he obtained a few fragments of it for their weight in gold. From such trials as the smallness of his specimens admitted, he considered it as a sulphuret of bismuth; by which name he described it in his Natural History of Minerals, published in January 1801, and reprinted in 1803.

SCIENTIFIC NEWS.

Middlesex Hospital.

Medical lectures.

MEDICAL Lectures, 1810—11, by Richard Patrick Satterley, M. D. Fellow of the Royal College of Physicians, Physician to this Hospital, and to the Foundling Hospital: and Thomas Young, M. D. F. R. S. Fellow of the Royal College of Physicians.

Dr. Satterley's course of *Clinical* instruction will begin the first week in November: the attendance on the patients will be continued daily, and lectures will be given once a week, or oftener, when it may be necessary, at 11 o'clock. Mr. Cartwright, surgeon to the Hospital, will undertake such occasional demonstration of morbid anatomy, as may be required for the illustration of the respective cases. The objects of the course will also be extended to such remarkable peculiarities in the diseases of *children*, as may occur in the *Foundling Hospital.* Terms of admission, to pupils of the Hospital, five guineas.

Dr. Young will begin, in February, a course of lectures on *physiology*, and on the most important parts of the *practice of physic*; in particular the nature and treatment of febrile diseases; he will deliver them on Tuesdays and Fridays, at 7 o'clock in the evening. Admission, two guineas: to former pupils, one guinea.

Those who are desirous of attending either of these courses, are requested to leave their names with the apothecary at the Hospital, from whom farther particulars may be known.

Popular lectures on the sciences.

The annual courses of popular lectures at the Surry Institution, Blackfriars Bridge, commenced on the 15th ult., and will be continued every succeeding Monday and Thursday evening, at 7 o'clock, during the season. We understand, that the following gentlemen have been engaged for the respective departments, viz.

Zoology, George Shaw, M. D., F. R. S.—Music, Mr. S. Wesley.—Zoonomy, John Mason Good, Esq.—The chemistry of the arts, F. Accum, M. R. I. A.—Natural philosophy and Astronomony, Mr. Hardie.

Lectures on electrical and electrochemical science.

Mr. Singer's lectures on electrical and electro-chemical science will recommence early in the ensuing season, at the Scientific Institution, 3, Princes Street, Cavendish Square. In these lectures a complete exposition of the subject will be given, and the illustration will be assisted by some new and interesting experiments. A prospectus of the plan of instruction may be had at the Institution, or of Mr. Cuthbertson, 54, Poland Street.

Investigation of the powers and properties of numbers.

Mr. Barlow, of the Royal Military Academy, has ready for the press, an Elementary Investigation of the Powers and Properties of Numbers, with their application to the indeterminate and diophantine analysis, to which will be subjoined a synopsis of all the most curious prpblems of this kind, selected from the best ancient and modern authors.

To Correspondents.

The paper of Messrs. Kerby and Merrick in our next.

METEOROLOGICAL JOURNAL,

For OCTOBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

SEPT. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night	9 A. M.	Day.	Night.
27	57°	60·5°	64°	53·5°	30·02	Fair	Fair
28	60·5	59	65	51	29·94	Ditto	Ditto
29	57	60	62	56	30·05	Ditto	Cloudy*
30	61·5	62	66	53	29·99	Ditto	Ditto
OCT.							
1	61·5	60	66	51	30·15	Ditto	Ditto†
2	57	55·5	62	49	30·25	Ditto	Fair
3	55·5	56	61·5	49·5	30·25	Ditto	Ditto
4	55·5	57	64	49	30·25	Ditto	Ditto
5	57	56	63	50	30·16	Ditto	Ditto
6	54	55·5	59·5	46	29·94	Ditto[a]	Heavy fog
7	50	56	59·5	49	29·96	Ditto[a]	Fair
8	54	58	59·5	52	29·97	Ditto[a]	Ditto
9	56·5	57	60	52	29·93	Ditto	Ditto
10	56·5	57	58·5	51	29·84	Ditto	Ditto
11	54·5	52	58	46	29·92	Ditto	Ditto
12	50	47	54	40	29·86	Ditto	Ditto
13	46·5	47	53	40	30·00	Ditto	Ditto
14	47·5	50	55	43	30·22	Ditto[a]	Ditto
15	49	52*	54	42	30·19	Ditto	Ditto
16	47	54	54	51	29·91	Rain	Rain
17	56·5	58	60	52	29·66	Ditto	Ditto
18	56·5	55	60	45·5	29·38	Ditto	Fair
19	53·5	59	59	57	29·80	Ditto	Rain
20	59	52	61·5	49	29·74	Ditto	Ditto
21	54	61	61·5	44	29·67	Ditto	Ditto
22	55·5	55	58	46	29·33	Cloudy‡	Fair
23	50·5	51	54	42	29·67	Fair	Cloudy
24	48	47	52	39	29·72	Ditto	Fair
25	44·5	45	49	37	30·17	Ditto	Ditto
26	42	45	50	42	30·37	Ditto	Ditto
27	40	45	48	38	30·23	Cloudy	Ditto

* Rain in the night

† Ditto

‡ Boisterous day and night

[a] Intervening fogs

Rain 1·745 inch since last Journal.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1810.

ARTICLE I.

On the Electric Column. By J. A. DE LUC, Esq. F. R. S.

PART III.

Concerning some Meteorological Phenomena, to the better knowledge of which it may lead as an Aerial Electroscope.

Necessity of circumspection in examining phenomena.

WHEN, in our researches, we have in view some great and determined object, we are not only more assiduous in our endeavours to approach it, but more attentive not to be misled in the road, and less disposed to be satisfied with mere surmises, while we perceive that some real discovery may be obtained by more circumspection. I shall therefore explain first, why every new *electric* phenomenon, which we encounter in the course of our experiments, must be attentively pursued and analysed in itself, and not connected with gratuitous hypotheses; for fear of losing a thread, which might lead us in the labyrinth of the *physical causes* acting on our globe, among which the *electric fluid* holds a high rank; as will appear by the following great object concerning this *fluid*, on which natural philosophers have not yet sufficiently fixed their attention, though it is explained in my former works.

Lightning.

It is commonly supposed, that the *electric fluid*, which, under the form of *lightning*, darts from certain *clouds*, existed previously in them, ready to be discharged, at a proper distance, on bodies which possess less of this *fluid*, either other *clouds* or the *ground*. On this idea, not improbable at first sight, Dr. Franklin founded his invention of *pointed conductors* elevated above houses, in hopes to preserve the latter from being struck by *thunder bolts*. With the above supposition, this method of security was very ingenious; for, if the *electric fluid* were *actually* accumulated in a *cloud*, ready to be discharged on the first part of the ground sufficiently elevated, a *pointed conductor* might discharge that *cloud* without a *spark*, as it does the prime conductor of an electric machine. But those who have frequently travelled on high mountains know certainly, that there is no analogy between a *thunder cloud*, and an *insulated body* on which *electric fluid* has been accumulated.

Conductors.

The electric fluid not accumulated in a thunder cloud.

A *cloud* is a mere thick *fog*, and thus such a completely *conducting* medium, that the most powerful electric machine worked in it could not, for an instant, accumulate the *electric fluid* on its *prime conductor*; it would be constantly diffused through that moist air, and lost in the surrounding bodies. This cannot be doubted; but it is supposed, that *clouds*, being surrounded by pure air, and thus insulated, can retain the *electric fluid* accumulated in them by whatever cause. In this consists the illusion, dissipated by what is observed on mountains. I have frequently been in valleys of the Alps, and of lower mountains, beset with *thunder clouds* leaning on both sides against *wet grounds*, and thus in so complete a *conducting* connexion with the mountains themselves, that it was impossible any accumulation of *electric fluid* could remain in the former; besides which, no cause of such an accumulation has ever been explained: however flashes of *lightning* were emitted from these *clouds*, with greater or smaller intervals, followed by the astonishing phenomenon of the *rolling of thunder*; and to suppose this to be the repetition of one *sound*, by *echoes* from *cloud* to *cloud*, is a fiction similar to that of poets or painters, who represent the gods as sitting on these fogs.

Lightning

Thunder and lightning

Lightning and *thunder*, when considered in their true nature, and with all their associated circumstances, though they are the most striking, have remained till now the most obscure of the atmospheric phenomena; and as at the same time their production is evidently connected with all the causes acting in the atmosphere, that great laboratory of nature on our globe, beginning from the very formation of *clouds*, this obscurity is spread over all the terrestrial phenomena. It is certain, by what I have above explained, that an instant before a flash of *lightning* strikes our eyes, no accumulation of *electric fluid* could have existed in *clouds* leaning against *wet* grounds: the sudden manifestation of this enormous quantity of *electric fluid*, not existing before *as such*, must therefore be the consequence of some *chemical* operation, depending on some new cause, which either disengages it from some *combination*, or generates it by some *composition*; and being thus instantly set free, it rushes in a torrent, before it can be diffused in the *cloud* and through this in the *ground*. Beside this immediate consequence of the certain fact, that the quantity of *electric fluid* thus emitted did not, the instant before, exist as disengaged in the *cloud*, various other phenomena, attending this effect, prove the existence of some great successive chemical processes, manifested principally by the successive *detonations* forming what is called the *rolling* of *thunder*: these are undoubtedly produced by concomitant *decompositions* and *recompositions* of still unknown atmospheric fluids, some producing the decomposition of the *air* itself, others proceeding from this first operation, as shall be explained hereafter.

the consequence of some chemical process.

Necessity of investigating atmospheric phenomena.

This is one of the greatest objects, that could be offered to the attention of natural philosophers: for it must strike them, that no system on the nature of *air* and *water* can have any solidity, if it happens to be in opposition to these grand effects produced, under our inspection, in the great laboratory of nature: and though our observation has not yet extended to all the atmospheric phenomena necessary to be embraced for the discovery of their specific causes, yet it is sufficiently advanced to indicate, according to general known laws, these *decompositions* and *recompositions* of

Atmospheric air not a mixture.

atmospheric air, as being a fluid *sui generis*, and not a mixture of two *aeriform fluids* differing in their nature, as has been concluded from specious phenomena produced in our experiments; but these phenomena I have explained in my works, without supposing such a mixture, in itself contrary to a number of atmospheric phenomena. This I shall here successively explain, though not with so many particulars as are contained in my works.

Rain.

Rain will be my first object; and indeed it ought to be so in every general system of chemistry, since no phenomenon, either spontaneous or artificially produced, is more connected with the manifestation of *water* in the modifications of *expansible fluids*; and none certainly is attended with greater consequences on our globe. With a view of supporting the new hypothesis of a certain *composition* of *water*, from which and its associate hypothesis of two distinct and defined *aeriform fluids* in the atmosphere, *rain*, so common a phenomenon, cannot be explained, the ancient and already exploded hypothesis of Mr. Le Roy, of *evaporation* being a *dissolution* of *water* by air, has been revived. This hypothesis, the only apparent resource of the modern theory of chemistry, was plausible at the time of its first publication, about 60 years ago, when meteorological observations were very little advanced; because it is certain, that *evaporation* restores, upon the whole, to the atmosphere, the same quantity of *water* as falls from it in *rain*, *dew*, and other aqueous meteors: but from a number of well determined phenomena, discovered by the progress of observation, this compensation is not immediate: that *water*, which ascends in the atmosphere by *evaporation*, passes through an intermediate state; undergoing a *chemical* transmutation, which makes it disappear to all our tests, sometimes for many months, it being then transformed into an *aeriform fluid*; and it must be by some inverse operation, that, all at once, *clouds*, *rain*, and the other concomitant phenomena are produced. I shall show hereafter how unfounded, as well as useless, is an hypothesis imagined for evading the consequences of these phenomena, which I have opposed to the new theory of chemistry; but first, I must proceed farther in the account of the phenomena themselves.

Evaporation supposed to be a solution of water in air:

but the water is changed into gas.

The

Meteors best studied among mountains.

The above consequences may be deduced from the most common atmospheric phenomena, even when only viewed from the plain, provided they are observed in all their consequences; but it is on high mountains, the very region of *meteors*, that, from other circumstances not perceptible in lower situations, the observer is induced to wish for more knowledge in the astonishing operations performed in this laboratory. Such has been the case with Mr. de Saussure and myself, on account of our frequent visits to the mountains of our native country, for the geological pursuits in which we were engaged at the same time. The surprising phenomena concerning *moisture*, which we observed in these high regions of the *air*, led us separately to the pursuit and construction of our respective *hygrometers*; in order to understand, by experiments and observations with this instrument, in what really consists *moisture* in the atmosphere; and to follow certain of its modifications, as its sudden increase and diminution without perceptible causes: a knowledge which, if not leading to immediate discoveries on the other atmospheric operations, might at least clear the way to these discoveries by dispelling and preventing errours.

Hygrometers.

Observations at variance with the supposed composition of water.

When our experiments and observations were first published, they attracted much the attention of natural philosophers; but by degrees they have been forgotten, from the increasing prevalence of the hypothesis of a *composition* of *water*, to which they were opposed, in consequence of their connexion with the most common meteorological phenomena; an opposition explained even before this hypothesis was so much relied upon as to effect a change in the whole nomenclature and language of chemistry.

This inattention, for a time, to real and important discoveries, an effect occasioned by prevailing prejudices, is observed under various forms in the History of Sciences; but there it is seen also, that an obstacle of this nature could not be perpetual, and it may be expected, that it will not be so in this case; therefore I shall here assemble some uncontroverted results of observation and experience, for the consideration of natural philosophers.

Theory of evaporation.

Article I. *Evaporation*, the original source of *atmospheric phenomena*, is not a *dissolution* of *water* by *air*, as is now so commonly assumed; *air* has no share in it. The immediate product of *evaporation* in all its stages, from the formation of *steam* by *boiling water*, down to the *evaporation* of *ice* in winter, is constantly and uniformly an *expansible fluid*, composed of *water* and *fire*, namely the *aqueous vapour*. This *fluid*, in whatever *temperature* it is produced, acts by *pressure*, in the same manner as the *aeriform fluids*, and in particular on the *manometer*, from the instant of its production, as long as it subsists; and the quantity of its production, attended with a proportional *pressure*, is the *same* in *vacuo* as in *air*, at its different *maxima* correspondent to each degree of *temperature*; a direct proof that *air* has not the smallest share in *evaporation*. Lastly, as long as this *fluid* subsists without any change in its nature, it never ceases to act upon the *hygrometer*, and its quantity is exactly measured by this instrument, with the addition of the *thermometer*. I have proved these assertions by the union of Mr. de Saussure's experiments and mine, in some papers published in the Phil. Trans. of 1793. It is evident, that, if these be real *facts*, the resource of the new theory of chemistry for explaining *rain* is overturned (as will be seen hereafter), and with it the theory itself; what then is the reason, that those, who still maintain it, remain silent on these *facts*? On this however rests (and will continue to rest till the contrary be proved by direct experiments) the whole of meteorology.

The maximum of aqueous vapour at every temperature a fixed quantity.

Art. II. Both Mr. de Saussure and myself have determined, by direct experiments related in our respective works, as I shall more particularly express hereafter, the *quantities* of *evaporated water* contained in one cubic foot of *air* correspondent to every degree of our *hygrometers*, at every *temperature*; and we have proved, that the *maximum* of this *water*, a *quantity* fixed for every *temperature*, cannot be exceeded, either by the increase of *water* in the same space, or by the diminution of *heat* with the same quantity of this *water*, without some of the *aqueous vapour* being decomposed, and *water* making its appearance by *precipitation*: and by my experiments it is moreover demonstrated, that no length of time, after the production of this *fluid*, can prevent either its effect on the *hygrometer*, or its remaining submitted in the same manner to the influence of *temperature*.

Art. III. The *aqueous vapour*, i. e. the immediate product of *evaporation*, is therefore never concealed in the atmosphere; and its *quantity*, in any part of the latter, can always be determined by the observation of the *hygrometer* and the *thermometer*. This *fluid*, produced by the *evaporation* that never ceases on the surface of the water and of the land, being of a specific gravity less than that of *air*, constantly ascends in the atmosphere, passing through its lower regions, where we do not find that it remains; it ought therefore to accumulate in the higher parts. Now, as we ascend on mountains, the *hygrometer* indicates less and less *evaporated water* in the transparent *air*. I shall soon answer the hypothesis, already mentioned as having been imagined for setting aside the conclusion which I have deduced from this phenomenon, namely, a transmutation of the *aqueous vapour* into *atmospheric air*; a conclusion however which will be found the ultimate result of this series of facts.

Aqueous vapour is always perceptible by the hygrometer.

Ascends,

but diminishes as it ascends

by conversion into air.

Art. IV. Another phenomenon, which Mr. de Saussure and myself have observed, proves, that *dryness* is still greater in the region of the atmosphere above the highest mountains, where it was natural to suppose, and I supposed it at first, that the *aqueous vapour* was accumulating. On plains and small hills, *moisture* is increasing in the *air* after sunset; and before we possessed our *hygrometers*, we had reason to suppose, that it was the same upon high mountains, for there also the *grass* becomes *wet*. This being the first common symptom of *moisture* observed after sunset, and even before, was one of the arguments in favour of the idea, that *dew* proceeds from the *ground*; but the *hygrometer*, that neglected inſtrument, has shown it to be a phenomenon belonging to the physiology of plants, and not to meteorology. On high mountains, while the *grass* on the ground becomes *wet*, the *hygrometer* being suspended at some height above the ground, in some insulated spot where the *air* is free, shows an increase of *dryness*, which continues during the night. I have determined the cause of this phenomenon by immediate observations; it proceeds from the condensation of the columns of *air*, while the *heat* diminishes in them; whence

Dew belongs to the physiology of plants.

Air on mountains driest in the night.

whence results, that the part of that *air*, which, during the day, rested on the *summits* of mountains, descending lower, is followed by the *air* which was higher before; and this, as long as the condensation continues in the lower parts, descending from higher regions, and thus passing over the *summits* in its way downwards, is found, in an increasing degree, *drier* than that which rested on them in the day.

Theories of dew.

Art. V. Among the atmospheric phenomena, that of *dew*, commonly considered as very simple, has been long, and is still now, an object of controversy among natural philosophers, who have not attended to the latest experiments and observations. The first and most plausible explanation was, that the *dew* descended from the *air*, by the condensation of the *evaporated water* spread in it, when *heat* diminishes; but some experimental philosopher, finding that this cause was not sufficient to explain all the circumstances of *dew*, conceived the idea, which I have above mentioned, that it ascends from the *ground*, because this retains longer the *heat* of the day, than the *air* above it; which circumstance was considered as increasing *evaporation*: both parties alleging in support of their opinion certain facts, which, though not denied, were not decisive. During the most active time of this controversy, about 60 years ago, I made with my brother various kinds of experiments and observations, which, by turns, favoured one or the other of these hypotheses, but neither of them decisively; and the question would have remained for ever in suspense, had not *hygrology* and *hygrometry* been pursued with the degree of attention and labour, that Mr. de Saussure and myself have bestowed upon them; from which the phenomenon of *dew* has appeared under a new and quite different aspect, which excludes both the above causes as fundamental in it, and shows why neither of them could explain its most essential circumstances.

Agreement of the experiments of Mr. de Saussure with the author's.

Art. VI. With respect to the experimental part, we have both determined, by direct and unconcerted experiments, the effects produced on our respective *hygrometers*, placed in a mass of *air*, wherein, the quantity of *evaporated water* remaining the same, there was no change but in the degree of *heat*. We have made the same kind of experiments on different quantities of *evaporated water* in the same *space;* and combining them, we have formed *tables* expressing the different effects of *heat* on *moisture,* correspondent to different quantities of *evaporated water* in the same *space,* and to the changes of *heat* in each of these quantities; from which *tables,* after having observed the *hygrometer* and the *thermometer* in any part of the atmosphere, the quantity of *evaporated water* contained in one cubic foot of that *air* is determined. These entirely distinct experiments have proved the constancy of the laws prevailing in these effects, by the astonishing agreement of our *tables,* though determined by very different instruments and processes: an agreement which I have shown in the already mentioned papers to the Royal Society.

Art. VII. This determination of the effect produced on *moisture,* i. e. on the indications of the *hygrometer,* by the changes of *heat,* in a mass of *air* wherein the quantity of *evaporated water* remained the same, was most essential in meteorology; and in particular it was indispensable for the decision of the question, whether the production of *dew* were principally owing to the *cooling* of the atmosphere; which appeared the most natural explanation, but on which however there were sufficient reasons of doubt to produce the obscurity which remained on this phenomenon; because nothing could be either determined or proved, concerning the real effect of the diminution of *heat* on *evaporated water,* without such experiments as above defined; and I come now to their immediate application to the phenomenon of *dew;* in consequence of some observations which were also separately made by Mr. de Saussure and myself. Towards *sunset* and in the beginning of the night, *moisture* increases in the *air* much more rapidly; and after *sunrise* and in the first part of the day, *dryness* increases also much more rapidly—in both cases, comparatively with the correspondent changes of *heat*—, than would be the case, did the same quantity of *evaporated water* remain in the *air.* This is a very succinct account of our experiments and observations concerning this object, the particulars of which may be seen in our respective works; but it is sufficiently distinct to allow me here to conclude, that thus has been pointed out one

Aqueous vapour in the air diminishes in the morning and increases in the evening.

one of the greatest questions and objects of investigation, concerning *terrestrial physics*, namely: what is the *cause* of the *disappearance* in the atmosphere of the greatest part of the *aqueous vapour* which it before contained, when the *sun* ascends on the horizon; and of the *increase* of its quantity, when the *sun* is setting; while the very reverse should have been expected from all the hitherto known causes? (as I shall show hereafter). To this investigation I shall now proceed as far as known phenomena will lead me.

Changes in the electric state of the atmosphere observed by Mr. de Saussure.

Art. VIII. I shall first mention a very important course of observations of Mr. de Saussure concerning the changes in the *electric state* of the atmosphere. He had erected a high *conductor*, in a favourable situation, on the brow of a hill in Geneva. The lower part of this *conductor* was connected with an insulated pair of pith *balls*, the divergences of which indicated the *differences* between the *electric state* of the upper air, and that in which the *balls* stood: he observed during many years the diurnal variations of this *difference*; and the main result of these observations is the following. In common weather, i. e. when no particular cause disturbs the course of the usual operations going on in the atmosphere during each period of 24 hours, the quantity of *electric fluid* increases in it from *sunrise* till some time in the afternoon; as is seen by the increase of a *positive* divergence of the *balls*. The new *electric fluid*, the formation of which is thus indicated, accumulates in the air, because it is transmitted but slowly to its lower part near the ground. But afterward, when the *hygrometer* shows a beginning of increase of *moisture* in the atmosphere, the divergence of the *balls* begins to decrease; and when at last *dew* is forming; the *electric equilibrium* is soon established between its upper and lower parts, the whole of the *electric fluid* formed in the day passing then into the ground. Now, it is during the first of these periods, that *dryness* increases in the atmosphere much more than would happen by the same increase of *heat*, did only the same quantity of *aqueous vapour* subsist in it as before *sunrise*; while on the contrary its quantity ought to increase by a greater *evaporation* being produced on the ground, which *dries* when heated by the sun. Hence it appears, that there is some connexion be-

The diminution of electric fluid in the atmosphere connected with the decrease of aqueous vapour.

tween the increase of the quantity of *electric fluid* and the dimmution of that of *aqueous vapour* in the atmosphere, during this period.

The electric fluid increases with the presence of the sun.

Art. IX. This points out, in the first place, a *formation* of *electric fluid* in the atmosphere, while the *sun's rays* pervade it. *Light*, the increase of which in the atmosphere is here the immediate cause, is certainly one of the *component* parts of the *electric fluid*; therefore, this *fluid* must be *composed* in some operation of nature on our globe. Now it is here already probable, that the *sun's rays*, in pervading the atmosphere, encounter in it the substances with which they compose the new quantity of *electric fluid* then manifeſted: and that, in general, they enter there into various combinations, is proved by their intensity being sensibly greater on the top of high mountains than in the lower parts of the atmosphere, as has been shown from experiments by Mr. de Saussure; which difference must proceed from their quantity being diminished in pervading the air.

The solar rays more intense on heights.

Farther instances of the effects of the composition and decomposition of the electric fluid.

Art. X. Some other experiments of Mr. de Saussure lead besides directly to this system concerning *compositions* and *decompositions* of *electric fluid*, as producing phenomena, the causes of which were unknown or mistaken. For instance, it has been found by experience, that, when water is poured upon an insulated plate of hot iron connected with an electroscope, this plate becomes *negative:* whence it had been concluded, that, when *water* is converted into *vapour*, it acquires a greater capacity for *electric fluid*; and thus deprives of a certain quantity of this *fluid* the body on which it evaporates. But Mr. de Saussure, having repeated the same process upon different heated bodies, found, that some, in particular silver, became *positive:* whence he concluded very naturally, that during the evaporation of water on hot iron some *electric fluid* was *decomposed*, and some on the contrary *composed* when the same operation took place on silver. He has also surmised, what I have since found by direct experiments related in my work *Idées sur la Météorologie*, that in the discharge of the *Leyden vial*, and in my experiments of the *magic picture*, the *spark* produces some diminution of the quantity of *electric fluid* on these bodies; which cannot be but by *decomposition*. It will successively be seen in what manner these previous remarks on the *electric fluid*, and the experiments on the same subject contained in my former paper and the first parts of this, are connected with meteorology.

Region of clouds and rain.

Art. XI. I have said, that, as we ascend mountains, the *hygrometer*, successively falling, indicates less and less *evaporated water* in the *air*. We thus however attain the region, in which *clouds* and *rain* are formed: and there it is, that the lessons of nature itself may guard us against the arbitrary dictates of imagination: I shall therefore relate what I have observed. At times when the atmosphere is so clear, that distant objects are seen very distinctly, and that the *hygrometer*, according to the tables that Mr. de Saussure and I have made from direct and separate experiments, does not indicate above two or three grains in each cubic foot of that *air*—small *clouds* may be seen forming in all parts of the very stratum of the atmosphere in which we stand, with very little or no *wind*. Sometimes, without any change in the *temperature* or *moisture* of the intermediate parts, these embryoes of *clouds* dissipate: but at other times they rapidly increase, unite together in the whole stratum in sight, and announce to the observer, that soon he will be enveloped by *clouds*. However, till the *clouds*, either moving towards him, or forming around him, occupy the very spot in which stand the *hygrometer* and the *thermometer*, he observes no sensible change in them: but the instant that a *cloud* envelopes him, the *hygrometer* arrives at its point of *extreme moisture*, and all the bodies are *wet*.

Formation of clouds.

Rain.

Art. XII. These preliminaries of *rain* often remain a long time, with only some variations, and at last dissipate without effect; and as soon as the *clouds* disappear in one spot, the *hygrometer* indicates the same *dryness*, as if no *cloud* had been there. But at last; though without any perceptible difference in the preliminaries, because some other *test* of the state of the *air*, beside those we possess, is *wanting*; the *clouds* increase in extent and thickness, above and below the place of observation, and *rain* is produced in more or less abundance. If *rain* be lasting, and at the same time in a great extent of country, it may happen either in a calm air, or during some regular *wind*. But when *rain* is partial and

with wind.

and in *showers*, sudden, and sometimes violent *winds* accompany these, arising from the *expansion* of the *air*, by its decomposition into *aqueous vapour*, in some place, while a *vacuum* is produced in other parts, by the resolution of that *vapour* into *rain*. Hence it is, that the direction of these *winds* is rapidly changing, and that they cease with the return of the transparency of the *air*. Lastly, in a *stratum of air*, which perhaps only half an hour before was *calm* and *transparent*, in which the *hygrometer* did not indicate any increase in the small quantity of *evaporated water*, and without any indication of increase of the quantity of *electric fluid*, some *clouds*, rapidly forming, produce *lightning*, *thunder*, *hail*, torrents of *rain*, and such violent *winds*, as tear up trees and overturn cottages on mountains.

Storms.

We may be for ever ignorant of the causes of these wonderful phenomena, but those who are aware that fiction, in the operations of nature, may lead to great errours, will prefer ignorance to a false science. As for me, from my first observations of these operations of unconstrained nature, and with the addition of a remark of Mr. de Saussure which I shall mention, I changed very essentially my former ideas on the atmospheric phenomena, as I have explained in my works, and shall repeat hereafter.

Fourcroy's hypothesis of a dry solution of water in air.

Art. XIII. In order to evade the general consequence, which, in my works, I have deduced from these facts, namely, that *rain* and the other concomitant phenomena are produced by different kinds of *decompositions* of the *atmospheric air*; which consequence is certainly the subversion of the new theory of chemistry; Mr. Fourcroy invented the hypothesis of a *dry* solution of *water* by *air*; supposing, that this *water* could no longer affect the *hygrometer*, which in consequence he discarded from the rank of a *meteorological instrument*; and having obtained the assent of many chemists, who have not applied to meteorology any more than himself, this instrument, so much wished for before by natural philosophers, is now hardly mentioned.

But this hypothesis, grafted on that of *Le Roy*, is in the first place absolutely *gratuitous*; no fact having been adduced in bringing it forward in chemistry against the positive facts contained in Mr. de Saussure's works and mine:

and besides it is of no avail, since Mr. Fourcroy himself, and all those who have adopted it, have been obliged to suppose, that this pretended *solution* of the *water* remains dependent on the *temperature*; which they are obliged to do, otherwise it would be nothing more than my system, with the appearance of refuting it. For, if the enormous quantity of *water*, which sometimes falls in *rain* from a very limited stratum of *air*, be not submitted to *precipitation* by the diminution of *heat*, it must have been changed into a *permanent* or *aeriform fluid*; and in the atmosphere no sensible quantity of any *fluid* of this kind exists, but the *atmospheric air*. Besides, since for this reason it is supposed in that hypothesis, that the *evaporated water* remains dependant on *temperature*, very little knowledge in hygrology is required to conclude, that it cannot cease to affect the *hygrometer* in proportion to its quantity, as is evident from Mr. de Saussure's experiments and mine. Lastly, with respect to that *fluid* the *decomposition* of which produces *rain*, its nature is clearly determined by the following circumstance: when we remain in a stratum of *air* till the end of the operations by which a deluge of *rain*, even with *lightning* and *thunder*, has been produced, the *residuum*, according to all tests, is the same *air* as before. Such are the objections which I have made to Mr. Fourcroy himself, and which he has not answered, or any chemist for him.

Storms do not alter the nature of the air to any chemical test.

Deception from observations on plains.

Art. XIV. These formations and modifications of *clouds*, when viewed only over head from the plain, have naturally inspired the idea, that by some cause the liberation and condensation, of *evaporated water* now and then take place in a great extent of the upper region of the atmosphere, which *water* descends and accumulates in the stratum of *air* where *clouds* form and produce *rain*. But this idea proceeds from a want of previous knowledge in hygrology, and of observations on high mountains: for in the first place, whencever, and from whatever cause that quantity of *water* may be supposed to proceed before any *precipitation* can take place, even in the first state of *vesicular vapour* which constitutes *clouds*, it must be preceded by *extreme moisture* in the still *transparent* air, since it is only the *excess* of that *water*, which is first precipitated in a *mist*; and when this *precipitation* ceases, *extreme moisture* still subsists in the *air*; as Mr. de Saussure and myself have found in all our hygroscopical experiments. Now, I have said above from observations on high mountains, that air is there *dry* till the moment before the formation of *clouds*, and that as soon as the *clouds* are dissipated, the *hygrometer* indicates the same *dryness* as before. This evidently shows, that the production of *clouds* and *rain* have their *cause* in the very *stratum* of *air* where they are manifested; and this *cause* cannot be any other than a *decomposition* of the *air* itself.

Clouds and rain from decomposition of the air.

Lastly, in these very *clouds*, which, being themselves a *conducting* mass, lean besides against mountains, it happens sometimes that *lightning* and *thunder* are produced; and this, as I have said before, without any previous sign of an uncommon quantity of *electric fluid* in them. This also points out some operation taking place in these *clouds*, by some modification in the cause which commonly produces a simple *rain*. The *electric fluid*, thus suddenly disengaged, must have been before in some chemical combination in the *air* itself, which prevented its manifestation, and is then destroyed. When we are above the *clouds*, we may see (as it has happened to me) *lightning* darting upwards, as it is commonly seen darting downwards when we are under the *clouds*; and even in this last case, we may judge that *lightning* is darted upwards, when we see only a great sudden light in *clouds*, without any *flash*, followed however by *thunder*.

Thunder and lightning.

The above are leading facts in the mass of atmospheric phenomena, certainly indicating the existence, in the atmosphere, of *subtile fluids*, beside those which have hitherto been discovered. This is the general object, which I am going to examine.

These facts evince the existence of subtile fluids in the atmosphere.

I shall here begin by explaining one of the results of my long labours in the pursuit of the *measurement* of *heights* by the *barometer*, of which all the steps are described in my work *Recherches sur les Modifications de l'Atmosphère*, published in 1772. My experiments and observations were first directed toward these two points. 1. To obtain, by a great number of observations at different measured *heights*, on mountains and towers, a *coefficient* expressive of *height*, to the

Investigation of the measurement of heights by the barometer.

the determined *law* of the *densities* of *air* correspondent to the differences of *pressure*, in a given *temperature* of the *air*. 2. An *equation* for the differences of actual *temperatures* with that fixed point. By dint of trials, I arrived so far in these determinations, that the method of measuring the height of mountains by the barometer has been found preferable to geometrical operations, on account of the impossibility of determining a law in the *terrestrial refractions*; beside the difficulty of finding proper bases for the triangles. But though the measurement of heights had been my first view in this undertaking, other modifications of the atmosphere became soon predominant in my pursuit, as I shall now explain.

In order to ascertain the degree of exactness, which could be obtained in the determination of the two parts of the *formula* above defined, I had measured the heights of 14 points above one another on Mount *Salève*, near Geneva; the whole height, above the level of the base that I had chosen, being above 3000 feet; and at each of these determined elevations I had made a great number of observations of the *barometer* in different temperatures, both in the same days and in different seasons. I had taken all possible precautions to ascertain the height of each of these points, in verifying the trigonometrical operation by levelling the whole mountain in passing by these points, in order that the *formula* might be applicable to other places: but had there been some inaccuracy in that respect, it could not affect the *coefficients* of the two *laws*, as applied to the same places; for if these *laws* had been sufficient, the *formula* would have assigned to them the same *height* by every observation.

Anomalies apparently from atmospheric phenomena.

Now, no *coefficient* to the *differences* of heights in the *barometer*, associated with any *equation* for the *differences* of the *thermometer*, could bring the *formula* to express the same *differences* of *height* between the same *points*: a proof, that the *two conditions*, with which alone the *formula* corresponds, do not embrace all the *causes* of variation in the *density* of *air*. Having however no other *data*, I fixed these two parts of the *formula* in the manner the most correspondent to the whole of my observations amounting to near 600; so that the *sums* of *anomalies* in *plus* and *minus* comparatively to the measured *heights* were equal; after which the *causes* of these *anomalies* became the object of my researches.

Expansion by heat not sufficient to account for these.

One of the principal means by which I had considerably reduced the former great *anomalies* in this measurement, which had appeared unconquerable, had been by introducing an *equation* for the differences of *expansion* of air produced by heat in the atmosphere. Considering therefore this effect of *heat*, which, by its increase, diminishes the *pressure* of columns of *air* of a determined *height*, under the same *pressure* of superincumbent air, as indicated by the height of the *barometer* at the upper station; and connecting this circumstance with the idea, that the *cause* of *heat* is an *expansible fluid*, namely *free fire*, which occupies in *air* a *space* without any sensible addition to its *mass*; I concluded, that some other *fluid*, for which we had not yet a *test*, as we have by the *thermometer* for the former, might be the cause of the above remaining *anomalies*.

The presence of aqueous vapour suggested itself;

This general conclusion brought into my mind the *aqueous vapour*, of which I knew that the *specific gravity* was much less than that of *air*; and supposing at that time, as is commonly thought, that its accumulation in the atmosphere was the cause of *rain*, I conceived that the difference of its quantity in different times must be very great, and that this might be the cause, or at least one of the causes of the *anomalies* I had in view.

and the state of the fixed barometer.

The same consideration led me also to a system concerning the remarkable, though not constant, correspondence of the *variations* in the sedentary *barometer* with *rain* and *fine weather*; as the same *fluid*, the abundance of which in the atmosphere diminishes its *pressure* upon the *barometer*, was supposed to produce rain. Having published this system in the above mentioned work, it obtained much approbation among natural philosophers, because no satisfactory explanation had been yet given of the above connexion of phenomena; however I did not intend to give it a full assent, till I had succeeded in the construction of a true *hygrometer*; judging already, that, without such an instrument, nothing could be determined with any certainty, concerning the modifications of *evaporated water* in the atmosphere. This

Aqueous vapour inadequate to account for the variations of the barometer.

judgment was soon confirmed; for Mr. de Saussure, who had made a quicker progress than myself in hygrometrical experiments, discovered the fallacy of the above plausible system, which at first he had adopted with applause: he demonstrated by direct processes, that, though the *aqueous vapour* is specifically *lighter* than *air*, the difference between its greatest and smallest quantity in the atmosphere at any time is so little, that it can explain but a very inconsiderable part of the *variations* of the *barometer*. I had not yet carried my own experiments so far, but I did not doubt the main result of his, as they bore all the characters of a true inquiry, and I abandoned my system, as applied to the *aqueous vapour*.

Probably other imponderable fluids in the atmosphere beside free fire.

But the general conclusion that I had deduced from the great reduction of *anomalies* in the measurement of heights by the barometer, which was principally owing to the introduction of an *equation* for the differences of the quantity of *free fire* in the air, still remained; namely, that some *expansible fluids* as *imponderable* as the former, hitherto unknown to us, might also account for that remarkable correspondence between the *changes* of the *weather*, and the *variations* of the *barometer*. For, the same *fluids*, which, from their abundance at certain times, lessen the pressure of the atmospheric columns on a certain extent of country, by dilating the *air* and repelling it to other parts, may also prepare its *decomposition*, for the production of *rain*, either alone, or accompanied with other *meteors*; and at other times they may be dissipated without producing any of these effects, occasioning only the fall of the *barometer*.

Meteorology intimately connected with the nature of the component parts of the atmosph.

The above series of facts and their immediate consequences present the greatest assemblage of operations of physical causes on our globe; and a general consequence certainly results from them, namely, that all these operations are so intimately connected with the nature of *aëriform fluids*, of *water*, *light*, *fire*, and *electric fluid*, that we cannot determine any thing with the smallest degree of certainty on the nature of any of these substances, without embracing the whole. When therefore we discover some new phenomenon of any of these *fluids*, at what distance soever this phenomenon may be from connecting itself with the operations which we observe in the atmosphere, it is not to be neglected; for we cannot arrive at any distant object, but by successive steps.

Hence attention to be paid to the aerial electroscope.

This is the consideration, that has induced me to fix my attention on the *electric* phenomena manifested by the instrument, which I have described under the name of *aërial electroscope*: as from the above atmospheric phenomena concerning *lightning* and *thunder*, which cannot leave any doubt, that they are produced by a certain *decomposition* of the *atmospheric air*; and from the correspondent circumstance of a formation of new *electric fluid* in the atmosphere, during the period of the day, when, the greatest part of the *aqueous vapour* vanishing in it, there remains scarcely any *ponderable* fluid but *atmospheric air*; it is manifest, that the *electric fluid* is one of the substances most intimately concerned in the *chemical* processes, which take place in the atmosphere, and on the nature of which it is the most important to acquire more knowledge.

Its movements unconnected with heat or moisture.

It may be seen in the *tables* of my observations of this new instrument, that the changes in the frequency of the *strikings* of the little *pendulum* have no determined connexion with those of either *heat* or *moisture* in the room; for though *heat* commonly increases in the course of each day, at the same time as the *frequency* of the *strikings*, nevertheless the former is not the cause of the latter; since with the same degrees of *heat* the frequency of the *strikings* is very different on different days. With respect to the correspondence of this phenomenon with the *variations* of the *barometer*, my observations have been too short for deciding any thing on this object, though I felt much interested in it; and besides, the *barometer* had but small variations during this short time, being always rather high. Therefore, this is a course of correspondent observations which remains to be followed.

Variations in the electricity of the ground and air.

The observations contained in the last *table* create a new interest in this pursuit, as they may become a mean of discovering the changes in the comparative *electric states* of the *ground* and the *air* near it. The little *pendulum*, by its *silver wire*, being placed in connection with the *zinc* side in these experiments, was therefore *positive*; and in this case (as well as when it is connected with the *copper* side) it must rise more rapidly towards the *ball* 18, in proportion as the *electric state* of the latter differs more from its own. We know that in the first case (that of my observations), when the *ball* 18 communicates with the *copper* extremity of the *columns*, it is *negative*, and thus differs from the *pendulum* as *negative* from *positive*; the *standard* of which, according to the important determination of Sig. Volta, is the *actual* electric state of the *ambient air*. Now, the observations contained in the above *table* show, that the *frequency* of the *strikings* is not always the greatest, when the *ball* 18 is undoubtedly *negative* by communicating with the *copper* side of the *columns*; it being often equal, and sometimes even greater, when the *ball* communicates with the *ground*. This is a remarkable phenomenon, showing, that sometimes the *ground* contains less *electric fluid* than the *air* near it, and it may in future lead to some important discovery concerning the operations going on at the surface of the ground depending on the atmosphere.

Sometimes the ground contains less electricity than the air near it.

This path but just entered.

I have employed much time and labour, to arrive at the entrance of this new road in the investigation of terrestrial phenomena: the *entrance*, I say, for I do not even consider it as completely open. With respect to the instrument itself, I may judge, that it is susceptible of farther improvements, both in the composition of the *column*, and in the machinery added to it: for in such a complication of new physical effects, and mechanical dispositions of parts, it is not to be expected, that every thing can be conceived by one individual. The very composition of the *column* might be improved with regard to the intensity of effect, by some other *metallic* coating than that of *copper* on *paper*, which I have employed on account only of its being ready prepared by using *Dutch gilt paper*. In some trials, I have found more effect in using *paper* covered with real *gold*, and with *silver*; and I have also found some advantage in doubling the *Dutch gilt paper*, by pasting a thin paper over its own paper. Many such trials may be made with a proper *condenser*, before whole *columns* are composed. As for the arrangement of the machinery connected with the *column*, the instrument which I have described having been

The instrument may be improved.

Remarks on the column.

successively augmented upon its original base, I suspect that it is too much crowded, and that thus its parts may have on each other an influence prejudicial to the effects, which I have remarked in other cases.

I have made many other remarks on this instrument, but my present purpose is more to engage other experimental philosophers in this pursuit, than to forward it myself; for with respect to these observations, I consider them as newly born. It will first require some time for the understanding what may be called the *language* of the instrument; i. e. its meaning as to the indication of the *electric state* of the *ambient air*, by its influence on the motions of the *pendulum*. This study has been opened too late for me, though I was engaged in it by considerations resulting from long *meteorological* observations, which, as they are of the greatest importance to natural philosophy, must be the incitement to this pursuit. Wishing therefore, that such observations may become a more general object of attention among natural philosophers, I have here endeavoured to show, by an abstract view of their present results, what knowledge, in following them, may be still obtained concerning the atmospheric operations. It is true, that observations of this kind require the neighbourhood of mountains (unless those who ascend in balloons should carry proper meteorological instruments, and apply themselves to these observations). But in general no real knowledge of the *nature* of the *atmosphere* can be obtained without, in some manner, ascending in it; and it is no less certain, that without this knowledge, no *chemical theory* can possess any certainty.

The indications of the instrument to be studied.

High parts of the atmosphere should be examined.

Systems are useful for promoting science, provided they be founded on all the knowledge already acquired respecting their object; but even then, as long as they contain *hypotheses*, they must be only considered as leading to new researches on determined points. With this view, I shall here conclude by an abstract of a meteorological system which I have fully explained in my former works, and especially in that under the title of *Introduction à la Physique terrestre par les Fluides expansibles.*

Systems useful.

1. During

Aqueous vapour transformed into atmospheric air by electricity.

I. During the time that the *sun's rays* pervade the atmosphere, the *aqueous vapour*, ascending in it by the *evaporation* which continually takes place on the surface of the earth, is transformed into *atmospheric air*, by some combination of this *vapour* with the *electric fluid*, which, during the same time, is formed in the atmosphere. A formation of *electric fluid* at that time is shown by Mr. de Saussure's observations already mentioned; but that the quantity thus manifested is not the whole, but that a great part of this new *fluid* is employed in the above transformation, is proved, as will be seen hereafter, by the production of *lightning* and *thunder*, which cannot have any other source.

Vector.

II. Thus, but by a particular operation, is formed that subtile *fluid*, which I have called *vector*, possessing many of the properties of light, but with the characteristic differences which I have determined. This fluid pervades instantly all bodies, is constantly present in the atmosphere, and has probably a great share in its phenomena; but its only function yet determined is, to unite with the *electric matter* composed at the same time; and, being thus the cause of the expansibility of the *electric fluid*, it produces all the phenomena known under the name of *electric influences*, as I have explained.

Dew.

III. In clear weather *dew* is produced at *sunset*, because that formation of *electric fluid* then ceasing in the *atmosphere*, the *aqueous vapour*, which continues to ascend in it, remains unchanged, and its quantity increasing too much in the air comparatively to the decreasing *heat*, it precipitates in visible particles of water: when *heat* decreases very rapidly in the air after *sunset*, the *vapour* is seen condensed as a *mist* over meadows; and at last in autumn it produces *fogs*.

Mist, and autumnal fogs.

Clouds and rain.

IV. The return of *atmospheric air* into *aqueous vapour*, whence result *clouds*, and afterward *rain*, is produced by some *subtile fluid* ascending from the base of the atmosphere, the *affinities* of which with the ingredients whereby the *aqueous vapour* has been transformed into *atmospheric air* decompose the *latter*. Thus, particles of *aqueous vapour* being substituted for particles of *air* in some *stratum* of the atmosphere, and becoming much too abundant to subsist in the same space, they first *precipitate* in the *vesiculæ* which form *clouds*; and if the *decomposition* of the *air* continues sometime in the same *stratum*, these *vesiculæ* collapse into *drops*, and form *rain*.

Variation of the barometer.

V. This is one of the causes of the *variation* of the *barometer*, not as a *prognostic*, but as a *consequence*. The absolute *mass* of the atmosphere is constantly changing by these inverse operations. When there is a long duration of *fine weather* over a great extent of country, the absolute quantity of *air* increases in the atmosphere, by the *aqueous vapour* which ascends in it continuing to be transformed into *air* during the day; and the *barometer* ascends, even in parts at some distance where it *rains*: when on the contrary there prevails over a great extent of country a long continuance of *decomposition* of *air* into *rain*, the mass of the atmosphere decreases, and the barometer falls, even in adjacent countries where there is *fine weather*. It is not therefore to be expected, that *rain* and *fine weather* should be positively connected with certain *absolute* heights of the *barometer*; its small motions, when it is more or less high, have the surest correspondence with the local weather; the fall indicating the presence of that *subtile fluid*, which tends to *decompose* the *air*, and the ascent, the cessation of this influence.

Rain alone.

VI. If, during the *decomposition* of *atmospheric air*, the *fluid* operating this effect so unites with the ingredients of the *electric fluid*, which had entered into the composition of that *air*, as to form a new *compound* in which the *electric fluid* does not possess its characteristic properties, *rain* only is produced, with little or no *electric* symptom; and this is the most common case. But when, from the nature of the new *fluids* which come to be spread in that stratum of the atmosphere, the decomposition of *atmospheric air* is such as to permit *electric fluid* to be produced by the precise ingredients (i. e. neither more nor less) necessary to its characteristic properties, it darts suddenly into the air in *lightning*: but this is only a first effect, and not yet *thunder*, a most astonishing phenomenon, consisting undoubtedly in successive *detonations*, such as the report of cannons fired in a rapid succession; and the former detonations must have

Lightning.
Thunder.

with the latter this analogy of cause, that they are *explosions* of a particular *expansible fluid*, produced by that kind of sudden decomposition of *atmospheric air*, as it happens by firing gunpowder and other processes.

Hail.

VII. A direct proof of these sudden *decompositions* of some substances in such *clouds*, and simultaneous *compositions* of other substances, is the production of *hail*. This effect shows, that, in a certain combination of circumstances, such a quantity of *free fire* enters suddenly into some *combination*, that the *freezing point* is much surpassed in the upper part of the *clouds*: hence the formation of grains of *sleet* so *cold*, that in falling through the *clouds*, their size is increased in the form of *icicles*, by the watery vesiculæ freezing upon them; of which formation the hail-stones bear all the characters, especially by having in the centre that opaque grain of *sleet*.

Sleet.

Other phenomena observable in the atmosphere.

The foregoing are the most conspicuous of the operations produced, at certain times, in some strata of the atmosphere, but not all those which an attentive observer may perceive: they are here, as must be the case in the first steps concerning all invisible processes producing visible effects, explained only by *general* analogies with known causes in our chemical processes; and if we cannot yet approach nearer to *specific* causes, it is because we are still very backward in the knowledge of these *subtile fluids*, which, at different times, come to mix with *air* in the atmosphere. We cannot however doubt, that to such *fluids* is owing the multitude of pheuomena still unexplained intelligibly, both in the atmosphere itself, and in its connexions with *vegetation* and the animal economy, when we consider what progress has been made in this knowledge, by only attending to the *chemical affinities* of *light* and *fire*, and by a beginning of discovery on those of *electric fluid*, the existence of which on bodies would be unknown to us, were it not for the *motions* produced in visible bodies, by the disturbance of its *equilibrium*: this is one of its characteristic properties, and our test of the degrees of its intensity in different cases; as the *thermometer* is for *free fire*, and *vision* for *light*.

Tests of the actual state of

These first steps in the knowledge of *causes*, which are themselves *imperceptible*, must render experimental philosophers

The air desirable.

sophers more and more attentive to all the circumstances, that may lead to the discovery, of new *tests* of the *actual* state of the *air*, in consequence of other *impalpable fluids* mixed with it; and also to the *electric* phenomena, that may appear in *chemical* processes, since the *electric fluid* is always present on the bodies which enter into *chemical* combinations, as it is present on all bodies: in this diffused state, it produces no known *chemical* effect; but all the phenomena before pointed out undoubtedly prove, that its *compositions* and *decompositions* have the greatest influence in the terrestrial phenomena.

Substances still unknown.

The general character of the system above extracted from my works already published, by supposing a multitude of still unknown substances, will undoubtedly encounter the disapprobation of those philosophers, who consider *simplicity* as the characteristic of the operations of nature: but if this word has any sense, it must signify *enough* and *nothing more*; therefore, the first condition is *enough*, and when, in certain phenomena, we find a deficiency of known agents, the chasm is not to be filled up by arbitrary hypotheses, which are nothing: *analogy* is our only sure guide in the investigation of hidden *causes*, as being a thread offered to us by nature itself.

Doctrine of Bacon.

This is one of the precepts of the father of true philosophy, the immortal Bacon, who taught us, in particular, not to dread the multitude of substances, when they are wanting for the production of phenomena accurately defined. Among his remarks on this subject is the following, under the 98th head of his *Silva Silvarum*; which remark I have the more admired, the longer I have studied the phenomena of our globe. "Cognitio humana determinata hactenus "fuit *speculatione* & *visu*; ita ut, quicquid oculos fugeret, "sive propter tenuitatem corporis, aut partes exiles, aut "subtilitatem motus, parum sit exploratum. Hæc tamen "naturam maxime regunt, illisque posthabitis, vera analysis institui nequit, aut indica rinaturæ processus. *Spiritus* aut *pneumatica* (expansible fluids) quæ omnibus "tangibilibus insunt, vix cognoscuntur . . . *Spiritus* enim "nihil sunt præter *corpora naturalia*, proportionaliter *rarefacta*, tangibilibus corporum partis inclusa velut tegu-

"mento: neque minus *inter se* differunt, quam *densæ* & *tangibiles* partes, omnibusque *tangibilibus corporibus* in-"sunt plus minusve, & plerumque nunquam cessant. Ab "his, eorumque motibus, præcipue procedunt arefactio, "colliquatio, concoctio, maturatio, putrefactio, vivificatio, "& præcipua naturæ effecta*."

Occult causes. Not to admit the existence of such substances, because they escape our *sight* or *touch*, would be returning back to *occult properties, essential qualities*, which, in the infancy of natural philosophy gratified the imagination under the shape of *causes*. However, these conceptions were a beginning of knowledge, as under that form were gathered a certain number of important *phenomena*, successively observed; but of these the *agents* were still to be sought for.

Imponderable substances, However it has been only at the birth of *pneumatic physics*, and when its progress has occasioned the investigation of the *chemical* affinities of *light* and *fire*, that many mysteries in nature have been unfolded; and what a field of new researches has been opened by the attention given to a third *imponderable substance*, the *electric fluid!* Now, these very great steps teach us, that no progress, marked by such memorable epochs, and followed by so many important consequences, can be expected but by farther discoveries in still to be discovered, the same class of substances, some of which may happen to manifest themselves also by characteristic effects, either known but mistaken, or yet unnoticed, and in these cases they might in some degree be submitted to analysis, by the changes they operate in certain phenomena, already known, but not sufficiently determined.

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* "Human knowledge has hitherto been guided by viewing and beholding; so that whatever escapes our eyes, either from the smallness of the body itself, the tenuity of its particles, or the subtility of its motions, is but little explored. By these however nature is chiefly governed; and if they be neglected, a just analysis cannot be made, or the processes of nature disclosed. The expansible fluids, that exist in all tangible substances, are scarcely known. These fluids are nothing but natural bodies, proportionally rarefied, included in the parts of tangible substances as in a case: nor do they differ less from each other, than the dense and tangible parts, they inhere more or less in all tangible bodies, and for the most part are never still. To these, and their motions, are owing in particular rarefaction, dissolution, concoction, maturation, putrefaction, vivification, and the principal effects of nature." C.

It

led to a further knowledge of nature,

It is not to be expected, that, by groping in a desultory manner among the objects of nature, any main road of investigation can be opened for the discovery of new *causes*; as their effects are so much intermixed in perceptible *phenomena*, that we cannot ascend to them with certainty in a retrograde manner. Many more discoveries concerning them may be expected from researches carried on by connected steps along the roads already opened in the maze of *imponderable* substances, the greatest agents in the phenomena of nature.

agreeably to what has already been effected.

The causes of heat,

electricity,

and magnetism.

The modifications of the *sun's rays* to produce *heat*, as followed by Mr. de Saussure and Dr. Herschel, and I may say by myself; as well as the first observations made by Dr. Priestley on the *chemical* effects of *light*, have opened one of these roads, which requires to be pursued in all its ramifications. Much is to be done also concerning the nature of *fire*, i. e. the cause of *heat*, or of that *expansion* of bodies of which the *thermometer* is the measure; a road which has been much obstructed by the obscure idea of *caloric*, introduced in the modern theory of chemistry, at the time when several experimental philosophers were engaged in researches concerning the nature, modifications, and combinations of the *expansible fluid* long known under the name of *fire*. Much more remains to be done in the study of the *electric fluid*, its production and decomposition throughout so many phenomena. Lastly, almost every thing remains to be done to acquire some knowledge of a *fluid*, the existence of which is manifested by some characteristic effects, but which is itself totally unknown; though it cannot be without some, and it may be a great influence in terrestrial phenomena: I mean the *magnetic fluid*, on which I shall say here only a few words.

Magnetism probably owing to a peculiar fluid, which has other effects.

Being now informed, that the *motions* of bodies occasioned by *amber* when it has undergone *friction*, of which the cause was unknown to the ancients, are the effects of a *fluid*, which has much greater functions in nature by its compositions and decompositions; when we come to consider the analogous, though much more limited effects produced by *steel bars* which have undergone proper *frictions*, we are led to conclude, not only that these particular *motions* are also the effects of a particular *fluid*, but that its functions in nature are not confined to those of attracting or repulsing *iron* according to circumstances, and producing in a movable *needle* the property of keeping more or less parallel to the meridian of the place, with a determined end pointing towards the north; though the latter, by its use in navigation, is become of great importance.

Its phenomena observed by Van Swinden.

With respect to this astonishing phenomenon, Prof. Van Swinden of *Francker* has much advanced what Bacon calls the *History* in every class of phenomena, by an indefatigable perseverance in observing the variations of the *magnetic needle* compared with various circumstances. This, for every phenomenon, is the first step toward the discovery of *causes*; for the nature of those that may be devised must answer to all the modifications of the phenomena carefully observed, before confidence can be granted to any hypothesis.

Its polarity particularly to be studied.

In *magnetism*, the main point which must direct the natural philosopher in search of a cause is the same which directs the navigator, namely the *direction* of the *magnetic needle*; for this must belong to a cause, which in some manner influences the whole Earth. This consideration has suggested to Prof. Prevost of Geneva an idea, which, though not completed, deserves notice. After all the discoveries already made in meteorology and chemistry, it cannot be doubted, that *light* has, in various ways, a great share in the formation of many *atmospheric fluids*, and thus probably of the *magnetic*: but there must be some cause of the formation of a greater quantity of it on the northern than the southern hemisphere of the Earth, since the *magnetic needle* has a tendency to turn that way. I shall not enter into particulars on Mr. Prevost's hypothesis, and shall only mention its ground, in order to show, that this object may not be unattainable; it is the circumstance, that the sun remains about 8 days longer on the northern side of the ecliptic, than on the southern.

Light probably concerned in it.

Saussure's magnetometer.

With respect to phenomena which may indicate a formation of this *fluid*, Mr. de Saussure has invented a very important instrument, which he has called *magnetometer*; showing variations in the intensity of *attraction* of a *magnet* in different parts of the day, and also in different days and seasons, as the *aërial electroscope* shows variations in the *electric state* of the air in the same circumstances. These two kinds of *variations*, therefore, deserve to be followed, comparatively with each other, and in their connexion with other atmospheric phenomena, as these observations may forward our knowledge respecting the *magnetic fluid*, which probably, as well as the *electric fluid*, by its *composition*, *decomposition*, and *combination* with other substances, has an influence in terrestrial phenomena.

The *loadstone* with respect to *magnetism*, and the *tourmalin* to *electricity*, are bodies which produce these phenomena from their own nature; but there is a method in our power to produce them by other bodies, namely *friction*: it is therefore very important, in either case, to discover in what manner *friction* acts to produce these effects. We have yet no hold in this pursuit with respect to *magnetic* phenomena, but some light may be reflected upon them by a determination of the manner in which *friction* produces *electric phenomena*. I have studied this subject with much attention, and I propose to relate, in another paper, some experiments of this kind, leading to the analysis of the *electric machine*, and demonstrating the errour of the idea of two *kinds* of *electricities*, or of two fluids acting in the *electric* phenomena.

Both magnetism and electricity naturally inherent in some bodies, and produced in others by friction.

Ashfield, near *Honiton*,
1st October, 1810.

II.

The results of ſome Experiments on the sonorous Properties of the Gasses, by Mr. F. KERBY *and Mr.* MERRICK, *jun., of Cirencester.*

To Mr. NICHOLSON.

SIR,

CONSIDERING the facility of procuring most of the gasses, it is, probably, in consequence of the difficulty of employing them for the purpose, that so few experiments have

Experiments on the sonorous properties of the gasses.

have been made on their sonorous properties. We have lately been occupied in making a few experiments on this subject, the results of which I send, in as concise a manner as possible, for insertion in your celebrated Journal, if you should think them of sufficient value. In some particulars they are very incomplete. We were prevented from determining the intensity of the sound by surrounding noises, and variable winds; but we purpose repeating and extending these experiments, at a more favourable opportunity.

Apparatus employed.

Our apparatus consists of a small pair of double bellows, fixed vertically in a wooden frame, having a brass screw underneath it, to fit into the plate of an excellent, single barrelled airpump. A thermometer is fixed against one arm of the wooden frame, and a small flute pipe of an organ (open at the end) against the other. A groove is made through this arm to convey the wind from the bellows to the pipe. See the dotted line Pl. VII, fig. 1. The whole is covered by a glass receiver, 13 inches high, and 7 in diameter; and the bellows are put in motion by turning backwards and forwards a bent wire, that passes through a collar of leathers at the top of the receiver, and is attached to another wire projecting from one end of a lever which has its other extremity fastened to the feeder of the bellows. Fig. 2. represents the pasteboard lining of the folds of the bellows.

Mode of making the experiments.

After 80 strokes of the piston, the pipe was inaudible: after 200 strokes, the gas was transferred into the receiver from a bladder in the usual way; and the bridge of the monochord was moved till the sound of the wire was perceived to be the octave below that of the pipe: then half the length of the vibrating part of the wire, in thousandths of the whole length, was set down in the fifth column of the following table. In the experiments *c*, *d*, *n*, *p*, *r*, (column 1) the gas was transferred in four nearly equal portions. The monochord was previously tuned by a *c* tuning fork, to Earl Stanhope's "first bass *c*"[*], Professor Chladni's

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[*] "Principles of the Science of Tuning," 1806.

Nicholson's Philos. Journal. Vol. XXVIII. [illegible] p. 4

Apparatus for Shewing the Sonorous properties of Gasses.

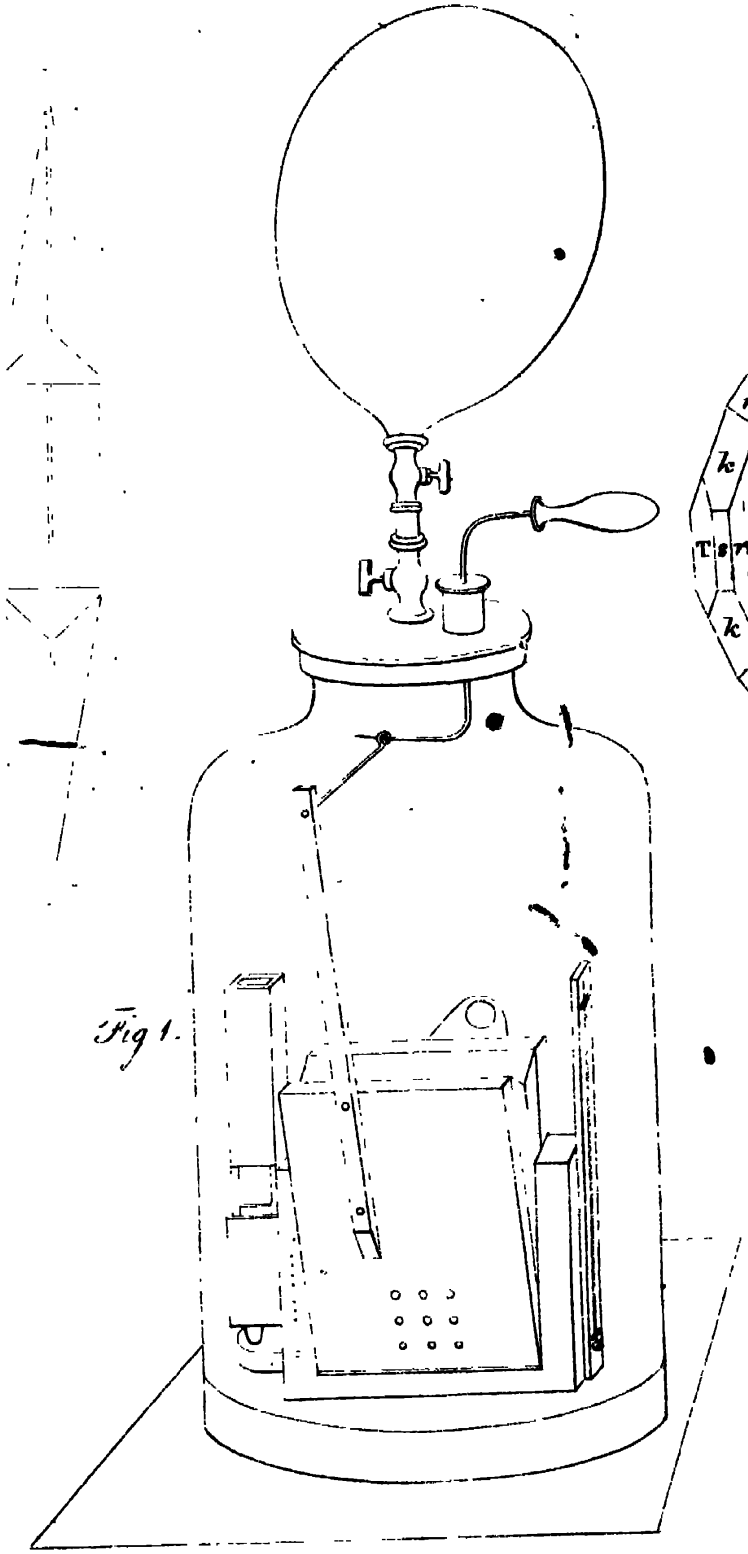


Fig 1.

Apophyllite

Fig. 3

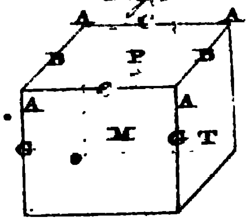


Fig. 5.

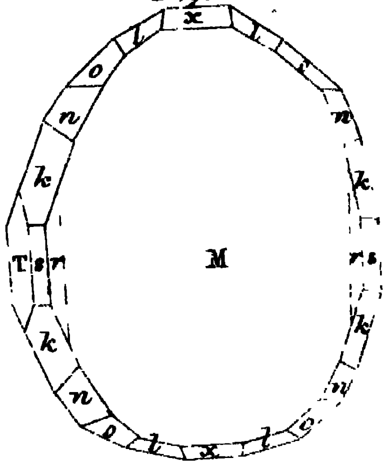


Fig. 4.

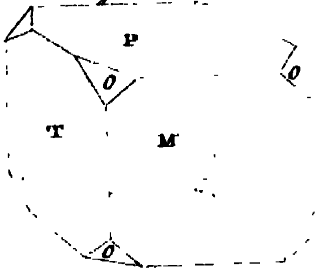
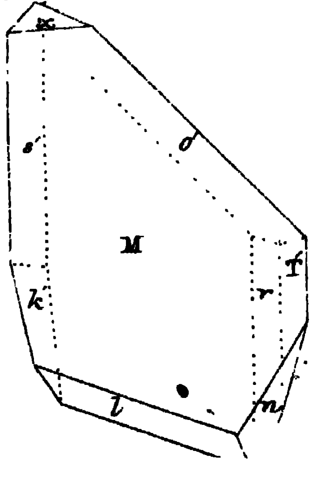


Fig 6.



ut 2†, or " *c* of the small octave," in the German notation‡.

I am, Sir,

Your obliged humble servant,

ARNOLD MERRICK.

Cirencester, October, 1810.

Tabulated results of the experiments.

Exper	*Barom*	*Therm*	*Aeriform fluids.*	*Monochord lengths*	*Distan. heard. Feet.*	*Remarks.*
a 1	29·69	57°.	Atmospheric air	·095	......	At midnight.
b 1	—	—	Oxigen gas	.100	......	From ox. of manganese.
c 1	—·68	60	Carbonic acid g.	·105	......	1st Portion.
2	—	—	———	·111	......	2d.
3	—	—	———	·112	......	3d.
4	—	—	———	·113	......	4th.
d 1	—	61	Hydrogen gas	·053	......	Obtained by means of zinc, &c.
2	—	—	———	·052	......	
3	—	—	———	·049	......	
4	—	—	———		......	
e 1	—	—	Atmospheric air	·093	......	
f 1	—	—	Nitrous gas	·100	......	From copper and nitric acid, &c.
2	—	—	———	·083	......	
3	—	—	———	·083	......	
g 1	—·56	—	Atmospheric air	·095	......	
h 1	—·53	66	Carbonic acid g.	·117	310	From marble, &c.
2	—	—	———	·115	342	
i 1	—	65	Atmospheric air	·095	273	
2	—	64	———	·095	1236	The receiver taken off the apparatus.
k 1	—·47	—	Ether vapour	·065	57	Every new dose of ether made the tone lower in pitch, at first.
l 1	—·38	63	Oxigen gas	·099	......	
2	—·37	—	———	·098	245	Strong wind.
m 1	—·36	61	Atmospheric air	·094	245	
n 1	—·49	65	Hydrogen gas	·047	......	Like the sound of a little bell.
2	—	—	———	·044	......	
3	—	66	———	·044	......	
4	—	—	———	·042	146	
1	—·48	70	Nitrogen gas	·089	......	
2	—	69	Hydrogen	·061	......	Full, smooth, sound.
3	—·45	66	No addition	·072	......	
4	—	—	Carbonic acid	·082	......	
5	—	—	Oxigen	·083	......	
6	—	—	Breath	·088	......	
p 1	—·44	—	Light carbu. hyd.	·088	......	Procured from wood.
2	—	—	———		......	
3	—	—	———	·089	......	
4	—	—	———	·090	341	
q 1	—	—	Atmospheric air	......	379	
r 1	—	—	Nitrous oxide	·108	......	Obtained from the Nitrate of Ammonia
2	—	—	———	·112	......	
3	—·43	—	———	·113	......	
4	—	65	———	·115	371	

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† Chladni's Acoustics.

‡ Callcott's Musical Grammar, p. 16. 1809. Dr T. Young's Lect. 2. 568. 1807.

III.

Description of the Apophyllite, Ichthyophthalmite of Dandrada and Reuss, Fischaugenstein of Werner. By Mr. Hauy *.

Fish's-eye stone considered as a zeolite,

THE mineral, which is the subject of the present article, appears to have been anciently known, and was classed as a species of zeolite, from its property of forming a jelly with acids. It had been analysed by Rinman, who mentions it by the name of zeolite of Hellesta, in Sweden. The results of his analysis are nearly the same as have lately been obtained by my celebrated colleagues, Fourcroy and Vauquelin, and by Mr. Rose, whom Prussia has recently lost, to the great regret of every friend of science.

or a feldspar.

Mr. Dandrada's description of this stone does not appear to me to mark it by characters sufficiently precise, to allow us to decide, whether it should occupy a separate place in the system, or be classed with some of the known species. Mr. Brochant, after having quoted the principal features of this description, adds, that the ichthyophthalmite appears to have several of the characters of feldspar: and the name given it by Mr. Dandrada accords with this analogy, the name being equivalent to that of fish's eye, which in the language of the old French mineralogists was applied to that variety of feldspar, which I call pearly, and which is the moonstone of our lapidaries.

to a species of which the name was formerly applied:

but it is a distinct species.

On examining some specimens brought hither about three years ago by Mr. Molir, I was convinced, that the ichthyophthalmite is clearly distinguished by its mineralogical characters, not only from feldspar, but from all other known minerals. I shall therefore proceed to detail these, which are already given for the most part in the work, which Mr. Lucas, jun., had drawn up from my public lectures, under the title of *Tableau méthodique des Espèces minérales.*

Its characters.

Essential character. Divisible into a rectangular parallelopipedon, having a triple tendency to exfoliation, by fire, by acids, and by friction *.

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* Journal des Mines, vol xxiii, p. 385.

Physical char. Specific gravity 2·467.

Hardness: not scratching glass, and giving no sparks with steel: scratching fluate of lime feebly, and carbonate of lime very evidently. If a fragment be rubbed sidewise on a hard substance, as if to polish it, it splits into leaves.

Refraction, simple.

Electricity, easily excited by friction. It is the vitreous.

Lustre. The surface of the crystals has a mean lustre between glassy and pearly, united with a transparency in general decided, without any proper colour.

Fracture, conchoidal, moderately shining.

Geometrical characters. Its primitive form, Pl. VII, fig. 3, is a quadrangular right prism with rectangular bases. The divisions parallel to M are very clear, and easily obtained. Those answering to P and T are not very evident except in a strong light †.

Chemical characters. Exposed to the flame of a candle, it splits into leaves. Before the blowpipe it fuses with difficulty into a white enamel. Immersed in cold nitric acid it divides in a few hours into small fragments, which at length become a white flocculent matter. Its powder forms in it a kind of jelly, similar to that produced under the same circumstances by the mesotype, or zeolite.

Analyses of the apophyllite.

	By Rinman,	Fourcroy and Vauquelin,	Rose.	
Silex	55	51	55	Component parts.
Lime	27	28	25	
Magnesia	0·5			
Potash		4	2·25	
Alumine	2·5			
Water	17	17	15	
	102	100	97·25	

---

* From this character I have taken the name of apophyllite, signifying, "a stone that exfoliates."

† The proportions of the three dimensions C, G, B, are those of the numbers $\sqrt{8}$, $\sqrt{9}$, $\sqrt{13}$.

Crystalline forms.

The specimens of apophyllite I have examined exhibit a few crystalline forms, among which the most simple is that seen on a groupe in the museum of natural history. It is the primitive parallelopipedon, the eight solid angles of which are cut off by triangular facets, *o*, *o*, *o*, fig. 4. The angle of incidence between *o* and M is 110° 50′.

Another variety, which I call supercompounded, is that represented fig. 5. The following are its principal angles of incidence. Between M and T, 90°: M and *s* 121° 57′: M and *r*, 149°: M and *k*, 118° 11′: M and *n*, 135° 32′: M and *o*, 110° 50′: M and *l*, 109° 32′: M and *x*, 119° 1′.

Very uncommon crystal.

The specimen, from which I determined this variety, is one of the most remarkable, that has come under my notice, since I began to study crystallography. It adhered in a single point only, as I may say, to its support, from which I separated it by a slight stroke. It followed from this position, that the crystal has its terminations on every side, which is itself not very common. But a still more uncommon circumstance is the contrast presented by all the parts similarly situate, when we compare them with each other. In general, when a crystal deviates from symmetry, it is only by the absence of a small number of facets, among those that are necessary to the integrity of the whole; so that these facets appear to have escaped the laws tending to produce them merely by accident, and the observer has little trouble to restore them in imagination. But in this crystal, which is represented exactly as it was formed fig. 6*, there is only one of the faces situate on one side, that has a corresponding face opposite to it: none of the other faces are repeated on the corresponding parts; and such is the progress of the decrements, that several of the faces which are single, as *o*, *n*, *k*, &c., ought to show themselves in eight different places, to leave no deficiency in the form of the crystal. It required time and study, to supply all these unexpressed circumstances of the crystallization, and reduce this sort of sketch, composed of ten faces seemingly without any connexion, to the real type of the form, which exhibits a well arranged assortment of forty-eight faces. It will be easy to perceive the connexion of this type with the crystal that includes only its elements, from the identity of the letters marking the corresponding faces on the two polyedrons, fig. 5 and 6*.

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* The faces T′, *o*′, *r*′, *s*′, *k*′, belong to the back part of the crystal.

Integrant particle.

Among the various forms of integrant particles, that are rectangled parallelopipedons, I know no one, that does not differ perceptibly from that of the apophyllite in the ratio of its dimensions, which alone is sufficient to show, that this mineral substance ought to be considered as a distinct species. On this subject I think it may not be amiss, to repeat what I have said elsewhere: it is not simply in the number and positions of the natural junctures, that the geometrical character consists, which I employ to distinguish one species from another, but also in the comparative dimensions of the forms of the particles. Hence arises a system of crystallization, which accords only with the substance possessing this form, unless it be a limit capable of belonging to several minerals, as the cube, regular tetraedron, &c.; in which case it is necessary to add an auxiliary physical character to that derived from the form of the particles, that the species may be determined unequivocally.

Haüy's rule for distinguishing species.

The analyses I have mentioned tend equally to establish an essential distinction between the apophyllite and all other minerals, and thus the results of chemistry and mineralogy with respect to this substance fully satisfy the two conditions enunciated in my definition of a species, considering this as an assemblage of natural bodies, the integrant particles of which are similar in form, and composed of the same principles united in the same proportions. "Mineralogy will have attained perfection, when we find throughout that conformity between the operations of two sciences, which should continually assist each other; and the agreement of which, as they investigate nature by very different paths, must doubly confirm the truths they disclose."

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* Fig. 6 represents the ten faces of the elementary crystal; fig. 5 shows only the twenty four faces of the complete crystal, supposed to be seen in front; but it is easy to conceive in imagination the other twenty four, which are on the back part.

IV.

On the Motion of Rockets both in Nonresisting and Resisting Mediums. By W. Moore, *Esq.; communicated by the Author.*

To Mr. NICHOLSON.

SIR,

SHOULD the following Essay on the Motion &c. of Rockets be considered sufficiently interesting for your valuable and well conducted Journal, you are at liberty to make use of it.

I am, Sir,
Yours very respectfully,
W. MOORE.

Royal Military Academy, Woolwich,
November 3, 1810.

The theory of rockets not investigated by mathematicians.

The theory of rockets is a subject, which has never yet engaged the attention of mathematicians; a circumstance which perhaps is partly to be ascribed to their not having been used until very recently as implements of warfare. The practice however of throwing them into besieged places, to cause their surrender, is now nearly universal among the English, and indeed is almost confined to them.

Their military use.

The invention of the military rockets* (as they are now called) as it regards the exemption of our troops from the enemy's power of annoyance, is to be esteemed as valuable. By the help of these machines the capital of Denmark and the well fortified town of Flushing, together with much of the French navy have within a few years been taken and destroyed with scarcely the loss of a single man: on which account, it is a matter of no small moment to bring the rules for discharging them and the methods of estimating their effects under various circumstances into one general and complete system; especially as their use is likely to become greater, and the improvements in making them extended.

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* The invention of the military rocket is exclusively due to William Congreve, Esq.; a gentleman well known and esteemed by the public for his ingenuity.

Mode of their projection.

The manner in which the military rocket is projected is from a case or long hollow barrel, fixed strongly to a frame or carriage like the several pieces of cannon : the rocket being first screwed to a lath or rod of ſome inflexible substance, to prevent any irregularity or perturbation of motion in it during its flight: and the elevation of the machine, from which it is thrown in any required case of devastation, belongs, like that of artillery, to an experienced practitioner.

Measure of the strength of their composition.

It is a matter of no consideration to the rocket engineer, to know the proportion of the several ingredients, with which the rocket matter is made, provided the *measure* of the *strength* of the composition be given. Such important *datum* in this very interesting theory I have not at *present* been able, for want of experiments, to ascertain; but it is presumed, that the force of the fluid generated from firing it cannot differ very much from that from fired gunpowder, which is about 1000 times as great, (according to Robins,) as the pressure of the atmosphere; and until I am able to convince myself otherwise, I shall adopt this as the measure of the strength of the rocket composition.

Before entering upon the several computations respecting the motion of rockets; it will not perhaps be wholly ungratifying to readers in general, to exhibit two or three of these machines, and give some little description of them.

The rockets described.

A C D B (Plate VIII, fig. 3,) is the case of the rocket of cylindrical figure and made of sheet iron; *a* the place where the rocket is fired at the base of it A B; and C G D is the head of the rocket in the form of a right cone, and filled with inflammable matter, that consumes much more slowly than that with which the case or body is filled. This head is made also of sheet iron, and is quite solid near the apex G, in order that it may the better enter any object of penetrable substance, as ships of war, and all buildings composed of combustible and yielding materials. The white spots in the head denote holes, through which the fire and flame rush and fire the building into which the rocket penetrates.

The head is about nine inches in length; the length of the case A C being about 2¼ feet.

When the head of the rocket is not used, as in the case of firing it among troops; then the machine is simply in the form of that represented by *Fig.* 4; having about ¾ of a foot of its length from C to E filled with grape and canister shot; at the extremity of which is a quantity of gunpowder E F *n m* to give them an additional impetus after the consuming of the wild fire; and by this means causing them to kill and terrify the enemy at a very great distance.

Investigation of their effects

This description of the military rocket being all that is necessary to my farther inquiries, I shall decline saying any thing more about it in this place, and proceed immediately to the investigation of their several effects; the nature and times of their motions; the angles at which they ought to be thrown, to fall upon an object at a given distance; and what ranges are within their sphere of devastation, &c. For all these are very important matters for military engineers and artillerists to know; to whom the management of them belongs, and whose object it therefore should be to prosecute such inquiries, and to render themselves masters of every particular, which the theory as well as the practice of throwing rockets embraces.

an object of importance.

Laws of their motion in a nonresisting medium.

On the Motion &c. of Rockets in a Nonresisting Medium.

Prop. 1.

PROP. I.

The strength or first force of the gas from the inflamed composition of a military rocket being given; as also the weight of the quantity of composition the rocket contains, together with the time of its burning, and the weight and dimensions of the rocket; to find the height it will ascend if projected perpendicularly, and also the velocity acquired at the end of that time; the laminæ of the composition being supposed to fire uniformly, and to burn parallel to the rocket's base.

Put $w =$ weight of the case of the rocket and head

$c =$ weight of the whole quantity of matter with which it is filled

$a =$ time in which the same is consuming itself uniformly

⊥ P

$n = 230$ ozs

$n = 230$ ozs. the medium pressure of the atmosphere on 1 square inch

$s = 1000$ times the pressure of the atmosphere; or force of the inflamed composition

$d =$ diameter of the rocket's base

$x =$ PD the space the rocket describes in the time t, and

$v =$ the acquired velocity in that time. Then,

ed^2 is equal to the area of the rocket's base (e being ·7854 the area of a circle the diameter of which is 1) and ned^2 the pressure of the atmosphere on a surface $= ed^2$. Hence $sned^2$ is the constant impelling force of the composition.

Now the weight of the quantity of rocket matter that is fired or consumed in the time t is $\frac{ct}{a}$; therefore $c - \frac{ct}{a}$ is the weight of the part unconsumed; which added to w gives $w + c - \frac{ct}{a} = m - \frac{ct}{a}$ (by putting $m = w + c$) for the weight of the whole mass at the end of the time t; or when the rocket has ascended to D; and so far as weight resists the motion of the rocket, this muſt be deducted from the impelling force. Hence $sned^2 - \left(m - \frac{ct}{a}\right)$ is the *motive* force at D; and $\frac{sned^2 - \left(m - \frac{ct}{a}\right)}{m - \frac{ct}{a}} = \frac{asned^2}{am - ct} - 1$ the *accelerative* force.

By the theory of variable forces we have generally $\dot{v} = 2gf\dot{t}$ (where f denotes the accelerative force and $g = 16\frac{1}{12}$ ft). Therefore $\dot{v} = \frac{2agsned^2\dot{t}}{am - ct} - 2g\dot{t}$; the fluent of which is $v = -\frac{2agsned^2}{c} \times \text{hyp.log.}\left(\frac{am}{c} - t\right) - 2gt$.

Now when $t = o$, $v = o$; therefore the fluent corrected will be $v = \frac{2gasned^2}{c} \times \left(\text{hyp. log.}\,\frac{am}{c} - \text{hyp. log.}\,\frac{am - ct}{c}\right) - 2gt = \frac{2agsned^2}{c} \times \text{hyp. log.}\,\frac{am}{am - ct} - 2gt$; which, when t becomes a, is $v = \frac{2agsned^2}{c} \times$ hyp. log. $\frac{m}{m-c} - 2ag$; or, because $m = w + c$, it will be $v = \frac{2agsned^2}{c} \times$ hyp. log. $\frac{w+c}{w} - 2ag$; which therefore is the velocity of the rocket when all the matter of inflammability in its body is just consumed.

This exemplified in numbers.

For an example in numbers, suppose the weight, dimensions, &c. to be as below; namely,

$s = 1000$
$n = 230$ ozs.
$w = 18$ lbs. $= 288$ ozs.
$c = 10$ lbs. $= 160$ ozs,
$a = 3$ sec.
$d = 3$ in. $= \frac{1}{4}$ ft.
$g = 16$ ft.
$e = \cdot 7854$

Then the above expression for v, namely $\frac{2agsned^2}{c} \times$ hyp. log. $\frac{w+c}{w} - 2ag = \frac{2 \times 3 \times 16 \times 1000 \times 230 \times \cdot 7854 \times \frac{1}{16}}{160} \times$ hyp. log. $\frac{448}{288} - 96 = 6774 \cdot 075 \times$ hyp. log. $\frac{14}{9} - 96 = 2992 \cdot 9895 - 96 = 2896 \cdot 9895$ feet, the velocity of the rocket per second at the instant of exhaustion of the wildfire.

To find the space x, we have by the doctrine of variable forces $\dot{x} = v\dot{t} = b\dot{t} \times$ hyp. log. $\frac{am}{am-ct} - 2gt\dot{t}$ $\left(\text{where } b \text{ represents the fraction } \frac{2agsned^2}{c}\right)$.

Method of finding the fluent for the time of the ascent of the rocket.

Now to find the fluent of this equation, we must first determine the log. of $\frac{am}{am-ct}$; which is done by first putting it into fluxions, and then finding its fluent in a series. Thus, the fluxion of the log. $\frac{am}{am-ct}$ being $\frac{c\dot{t}}{am-ct}$, we shall by expanding the fraction and taking the fluent of each term have, for the log. $\frac{am}{am-ct}$ the series $\frac{c}{am} \times \left(t + \frac{ct^2}{2am} + \frac{c^2t^3}{3a^2m^2} + \frac{c^3t^4}{4a^3m^3} + \frac{c^4t^5}{5a^4m^4} + \&c\right)$. Hence the above fluxional expression becomes $\dot{x} = \frac{bc}{am} \times \left(t\dot{t} + \frac{ct^2\dot{t}}{2am} + \frac{c^2t^3\dot{t}}{3a^2m^2} + \frac{c^3t^4\dot{t}}{4a^3m^3} + \frac{c^4t^5\dot{t}}{5a^4m^4} + \&c\right) - 2gt\dot{t}$; whose fluent is $x = \frac{bc}{am} \times \left(\frac{t^2}{2} + \frac{ct^3}{6am} + \frac{c^2t^4}{12a^2m^2} + \frac{c^3t^5}{20a^3m^3} + \frac{c^4t^6}{30a^4m^4} + \&c\right) - gt^2$, which wants no correction: therefore in the case where $t = a$; $x = \frac{bc}{am} \times \left(\frac{a^2}{2} + \frac{ca^2}{6m} + \frac{c^2a^2}{12m^2} + \frac{c^3a^2}{20m^3} + \frac{c^4a^2}{30m^4} + \&c\right) - a^2g = \frac{abc}{2m} \times \left(1 + \frac{c}{3m} + \frac{c^2}{6m^2} + \frac{c^3}{10m^3} + \frac{c^4}{15m^4} + \&c\right) - a^2g$; the space through which the rocket ascends during the time of its burning.

Hence retaining the numbers in the example above for the velocity, we shall have $x = \frac{6774{\cdot}075 \times 3 \times 160}{2 \times 448} \times \left(1 + \frac{160}{3 \times 448} + \frac{160^2}{6 \times 448^2} + \frac{160^3}{10 \times 448^3} + \frac{160^4}{15 \times 448^4} + \&c\right) - 144 = 3628{\cdot}96875 \times 1{\cdot}14622279$ (the sum of the series to 6 terms) $- 144 = 4159{\cdot}606684 - 144 = 4015{\cdot}606684$ feet, the space the rocket ascends through during the 3 seconds it is on fire.

Another method of determining the fluent.

The fluent of $b\dot{t}$. hyp. log. $\frac{am}{am-ct} - gt\dot{t}$, might indeed have been obtained without a series; for $b\dot{t}$. hyp. log. $\frac{am}{am-ct} = b\dot{t}$. hyp. log. $am - b\dot{t}$. hyp. log. $(am - ct)$ the fluent of the former part of which is evidently bt. hyp. log. am; and the fluent of $\dot{t}$. hyp. log. $(am - ct)$

$= t$,

$= t.$ hyp. log. $(am - ct) +$ fluent of $\frac{c t \dot{t}}{am - ct} = t.$ hyp. log. $(am - ct) - t - \frac{am}{c}.$ hyp. log. $(am - ct) = \left(t - \frac{am}{c.}\right).$ hyp. log. $(am - ct) - t = -\frac{1}{c}(am - ct).$ hyp. log, $(am - ct) - t.$ So that the whole fluent will be $x = bt\,.$ hyp. log. $am + \frac{b}{c}(am - ct).$ hyp. log. $(am - ct) + bt - gt^2$ which when $x = o$, and $t = o$ is $\frac{bam}{c}.$ hyp. log. $am.$ Hence the fluent corrected is $x = \left(bt - \frac{bam}{c}\right)$ hyp. log. $am + \frac{b}{c}(am - ct).$ hyp. log. $(am - ct) + bt - gt^2$, and in the case where $t = a$ it is $x = \left(\frac{abc - abm}{c}\right)$ hyp. log. $am + \frac{ab}{c}(m - c).$ hyp. log. $(am - ac) + ab - a^2 g = (c - m).$ hyp. log. $am + (m - c).$ hyp. log. $(am - ac) + c - \frac{acg}{b} = \frac{ab}{c}\left((m - c).\right.$ (hyp. log. $(am - ae) -$ hyp. log. $am) + c - \left.\frac{acg}{b}\right) = \frac{ab}{c} \times \left((m - c).\text{ hyp. log. }\frac{m - c}{m} + c - \frac{acg}{b}\right).$

This in numbers is $= 127{\cdot}0139 \times (288 +$ hyp. log. $\frac{9}{14} \times 160 - 1{\cdot}133734) = 4015{\cdot}9827734.$ So that it appears that by summing the foregoing series only to 6 terms gives the result within ·376089 part of a unit, of this method.

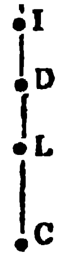
Height to which the rocket would ascend if the retardation from the force of gravity were constant.

Since we have found the velocity at the end of this space to be 2896·9195 feet per second; we shall, on the supposition that the retardive force of gravity remains constant from D have by the theory of uniform forces $\frac{v^2}{4gf} = \frac{\overline{2896{\cdot}9895}|^2}{64 \times {\cdot}9993709} =$ 131261·131 feet for the height to which the rocket will farther ascend; which being added to that just determined 4015·9827735 ft. gives 135277·1137735 feet, for the whole height of the rocket above the surface of the Earth when it has just lost all its motion, which is nearly equal to 27 miles.

Height of the real ascent according to the true law of this retardation.

But if the height to which it will farther rise be demanded on the true principle, that gravity varies inversely as the square of the distance from the Earth's centre; Then,

Putting $r = $ C L the rad. of the Earth
$a = $ C D the diſtance of the point to which the rocket has already ascended from the centre C
$x = $ C I any variable distance from C
$v = $ velocity at I
and $c = $ velocity at D $= 2896 \cdot 9895$ ft.



Then $x^2 : r^2 :: 1 : \frac{r^2}{x^2}$ the retardive force of gravity at I when that of the surface L is considered as unity.

Hence $-v\dot{v} = 2gf\dot{x} = \frac{2gr\dot{x}}{x^2}$ (the negative sign being used because the velocity decreases) whose fluent is $v^2 = \frac{4gr^2}{x}$, which, when $x = a$, and $v = c$, is $c^2 = \frac{4gr^2}{a}$; therefore the fluent corrected will be $v^2 = c^2 + \frac{4gr^2(a-x)}{ax}$:

So that when $v = o$, we shall have $c^2 + \frac{4gr^2(a-x)}{} = o$, and $x = \frac{4agr^2}{4gr^2 - ac^2} =$ (taking the Earth's radius at 3979 miles) 21145143·65521 feet, the whole height of the rocket from the centre of the Earth; and consequently $21145143 \cdot 65521 - r = 136023 \cdot 65521$ feet is the whole height from the surface. Whence also the height to which the rocket rises from the point where the impelling force of the composition ceases or is destroyed is 132007·67221 feet.

Hence it appears, that, in consequence of the diminution of the force of retardation from gravity upwards according to the inverse square of the distance from the

Earth's centre, the rocket will ascend nearly 746.54121 feet higher from a point 4230·609 feet above the Earth's surface with a velocity of 2896·9895 feet per second, than it would do if the same force as at the point D had continued constant, or had continued to act upon the body always with the same intensity. Hence also, if the rocket had a velocity of 2896·9895 feet per second upwards when at a height from the Earth's surface $= \frac{4gr^2}{c^2} - r$, it would never return; but continue to move for ever, or fly off to an infinite diſtance. For the expreſſion for x is $\frac{4agr^2}{4gr^2 - ac^2}$, or $x = \frac{4agr^2}{4gr^2 - ac^2}$; where it is evident that on ac^2 becoming $= 4gr^2$, x will be infinite; and therefore to find a, we have only to put $4gr^2 - ac^2 = o$ and reduce the equation.

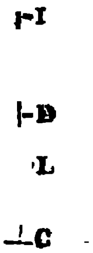
Force that would prevent its returning.

Whence, having the height from which the body muſt fall to acquire a velocity, which, being added to that of 2896·9895 feet per second, shall cause it to move perpetually when projected with the velocity of their sum; we can readily determine what that velocity is; and it being a very curious fact to know, we will therefore give a solution to the problem in this place,

Put $d = \frac{4gr^2}{c^2} =$ C I the given height from the centre C

$x =$ CD, any variable height from the same point greater than the rad. C L

$r =$ C L



Then $\frac{r^2}{x^2}$ is the accelerative force of gravity at D when that at the ſurface is 1. Therefore $v\dot{v} = -2gf\dot{x}$; and the fluent of the same is $v^2 = \frac{4gr^2}{x}$; which when properly corrected is

$$v^2 = 4gr^2\left(\frac{1}{x} - \frac{1}{d}\right) = (\text{when } x = r)\ 4gr^2 \times \left(\frac{1}{r} - \frac{1}{d}\right)$$

$= 4gr^2 \left(\frac{d - r}{dr}\right) = \left(\text{because } d = \frac{4gr^2}{c^2}\right) 4gr^2 \times \frac{4gr - c^2}{4gr^2} = 4gr - c^2$. Therefore the velocity acquired in descending through $d - r$ is $v = \surd 4gr - c \doteq 36553{\cdot}3482$ feet per second; which, added to the given velocity 2896·9482 feet per second, gives 39450·2377 feet, or 7·471768 miles for the velocity of projection to cause a body to move to an infinite distance.

To be continued.

V.

Remarks on a new Principle introduced by LEGENDRE *in his Elements of Geometry. In a Letter from* THOMAS KNIGHT, *Esq.*

To Mr. NICHOLSON.

SIR,

MR. LEGENDRE in his "*Eléments de Géométrie*" (p. 311, 3d Ed.) has attempted to demonstrate certain propositions, by a new and very peculiar kind of reasoning; founded on the consideration of functions, and the homogeneity of quantities.

New mode of reasoning by Legendre,

The principle, introduced by this eminent geometer, appears to have been favourably received by his own countrymen; but has of late been alternately praised and censured by some of our writers: though no very convincing arguments have been advanced on either side of the question.

favourably received in France, and by some in Britain,

If you can afford me a place in your valuable Journal, I will endeavour to prove the fallacy of Mr. Legendre's reasoning; first, by showing, that it would lead to the most absurd conclusions; and, secondly, by clearly pointing out the errour in this mode of investigation.

but it is fallacious.

The

Proof of this.

The principle itself, as well as the small reliance that can be placed on it, may be understood from the following

*Theorem**.

If two sides of one triangle are equal respectively to two sides of another, the third sides also are equal.

For let A and B be two sides of a triangle, p the included angle, C the opposite side. If A, B, and p be given, C will evidently be completely determined. C therefore is a function of A, B, and p. But it is plain, that p cannot enter into this function: for let some line, as D, be represented by unity: then A, B, and C are numbers, and if there could be an equation between A, B, C, and p, we might find p in terms of A, B, C; whence p would be a number, which is absurd. It follows from this, that C is a function of A and B only; whence the truth of the proposition is manifest. Q. E. D.

It is needless to say, that the principle must be erroneous, which leads to *such* a conclusion.

Things that cannot be compared not therefore independent of each other.

A writer in the *Edinburgh Review*† asserts, that this reasoning "takes for granted nothing, but that an angle and a "line are magnitudes, which admit of no comparison." It is a sufficient answer to this; that the quantity of grain in our barns, and the weather which preceded the collecting it there, are quantities which admit of no comparison; and yet the former has a pretty evident dependance on the latter.

Use of the word function.

It may be observed in the above proposition, that the term *function* is used in a very restricted sense: merely denoting numerical equality, or at most equality of homogeneous quantities; whereas every one knows, that a quantity may be a function of (or dependant on) another, without any such *abſolute* equality as is there supposed.

Fallacy of Legendre's reasoning

It is of no use therefore, to have shown, that there can be no equation, *properly so called*, as $C = \varphi\,(A, B, p)$ between A, B, C, p, unless it could further be proved, that there can be no analogy as $C \propto \varphi\,(A, B, p)$ between the same quantities. This may be very simply exemplified;

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* This differs from the first of Mr. Legendre's only in this; that I have changed angles into sides, and the side into an angle.

† No 29, p. 4.

In the sector of a circle, the angle at the centre is a function of the arc and the radius; viz: Angle $\propto \frac{\text{Arc}}{\text{Radius}}$; but this is no equation, except we make an arbitrary choice of units.

Does not almost the whole of dynamics consist of equations (or to speak more properly analogies) between *heterogeneous* quantitities? But I imagine the falsehood of this kind of reasoning is already sufficiently proved.

I am, Sir,

Your most obedient servant,

THOMAS KNIGHT.

Papcastle, Nov. 3d, 1810.

VI.

Description of an Hygrometer for Gasses, and the Method of Using it, to subject different Substances to their Action: *by Mr.* Guyton-Morveau[*].

Ascertaining the dryness of gasses important.

SINCE philosophers have endeavoured to investigate the properties of aeriform fluids, various kinds of apparatus have been invented, for placing them in contact with substances, that by their action on them might elucidate their nature, or might form with them new combinations. Of late they have particularly felt the necessity of reducing gasses to the greatest dryness, that the phenomena produced by their essential constituent parts might not be confounded with those, that might result from the decomposition of the water given out by them.

The instrument repeatedly tried.

The instrument, which I now lay before the class, appears to me well adapted to both these objects. It was not till I had several times tried it, that I resolved to have one finished with great care, and to communicate a description of it to those, who are aware that in these delicate experiments we have never too many means of securing ourselves against the errours of manipulation.

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[*] Annal. de Chim. vol. LXVIII, p. 5. The instrument here described was laid before the physical and mathematical class of the Institute on the 8th of August, 1808.

Description of it.

This apparatus being intended to be introduced, very accurately closed, under a receiver the mouth of which is immersed in mercury, it must be mounted wholly in iron. When the vessel is carried above the level of the mercury, it is easy to open it, to give an opportunity for the mutual action of the substance it contains and the gas under the receiver; which is effected by moving the dependent part of the counterpoise, previously disengaging the catch. To keep it in this position, nothing is required but a simple wooden bracket, with a notch cut in it to receive the handle of the instrument, in which it is fixed by a wedge. After having allowed it to stay as long as is necessary for the action required to take place, the glass vessel may be closed by its glass stopple, and taken out of the trough, without danger of any thing escaping from it, or of a single particle of mercury getting in, by means of the same catch, which is placed in the middle of the counterpoise, and strongly presses down the cover.

Method of using it.

To examine the hygrometrical state of any gas, take the glass bottle out of its collar, weigh it accurately, and fill it with dry pulverized muriate of lime, that has been in fusion; which likewise must be weighed. Having replaced it, and shut down the cover close, introduce it under the receiver, and then raise the cover. The weight gained by the muriate of lime will indicate the quantity of water absorbed.

Explanation of the figures.

Explanation of the plate.

Pl. VIII, fig. 1, represents the apparatus introduced under the receiver, the glass stopple raised by the hinged cover, to which it is cemented. The cover is kept up by the catch *g*; which in this situation is placed under the cross part of the handle. This figure is on a scale of two lines to an inch, English measure.

In fig. 2 the essential parts of the instrument are shown on a scale of four lines to an inch.

A is a glass bottle, holding two or three centilitres, the mouth of which is ground perfectly even; and confined by the screw *d* in the collar *B*, which opens with a joint at *C*,

Guyton-Morveau's Hygrometer for Gasses.

Fig. 1.

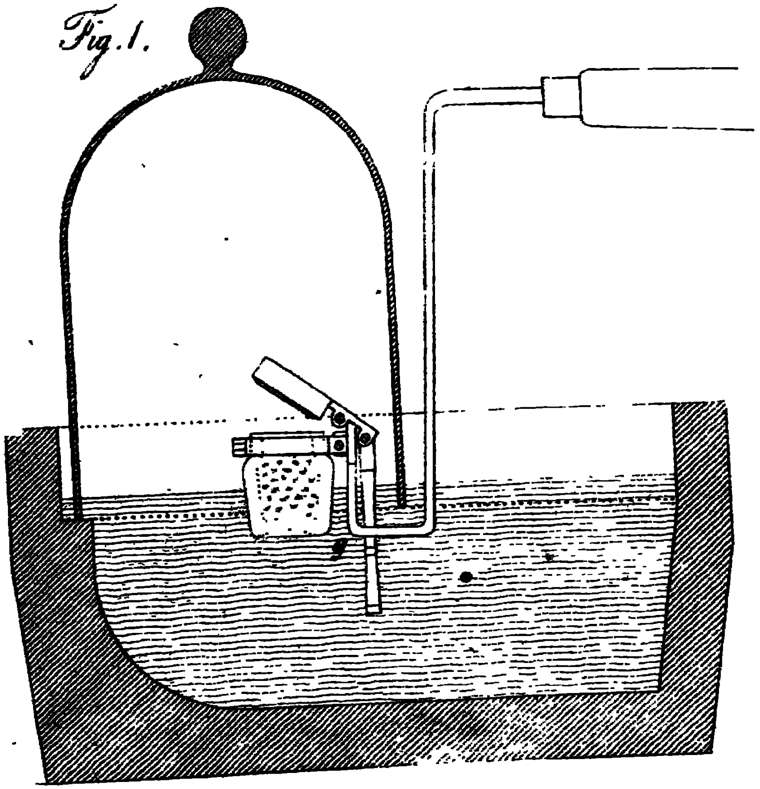
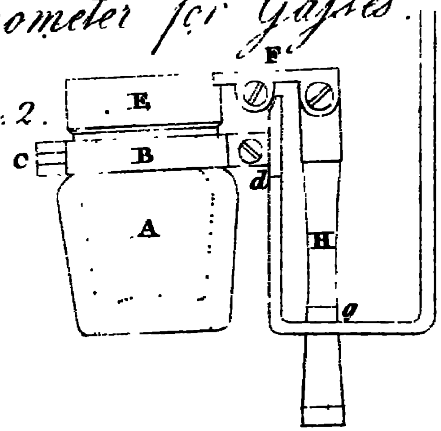


Fig. 2.



Military Rockets

Fig. 3.

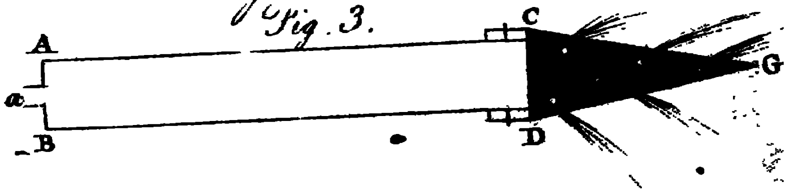
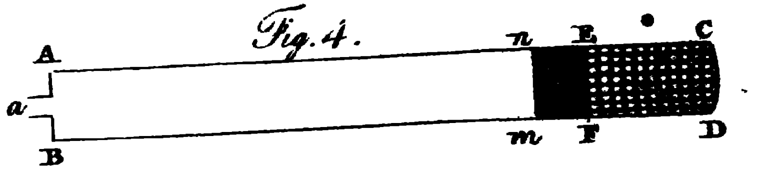


Fig. 4.



so that the vessel may easily be taken out, to be cleaned or weighed.

E the cover, to which the glass stopple is cemented. It is here seen closed by the position given to the counterpoise *F*, which is secured by the pressure of the catch *g*, in the moveable part of the counterpoise *H*, on the elbow in the handle.

VII.

The Croonian Lecture. *By* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.*[*]

Croonian lecture

I AM aware that the remarks, which I have to offer on the present occasion, may be thought to bear too little direct relation to each other for insertion in the same lecture; yet any observation respecting the mode of action of voluntary muscles, and every inquiry into the causes which derange, and into the means of assisting the action of the heart and blood vessels, must be allowed to promote the design of Dr. Croone, who instituted these annual disquisitions. And it has always appeared to be one great advantage attending the labours of this society, that it favours the production of any original knowledge, however small, in a detached form; and enables a writer to say all that he knows upon a particular subject, without inducing him to aim at the importance of a long dissertation.

One of the advantages of the society.

Subjects of this paper.

I shall therefore make no apology for dividing the following lecture into three distinct parts.

In the first of which I shall treat of the duration of voluntary action.

In the second, I shall attempt to investigate the origin of seasickness, as arising from a simple mechanical cause deranging the circulation of the blood.

In the third, I shall endeavour to explain the advantage derived from riding and other modes of gestation, in assisting the health under various circumstances, in preference to every species of actual exertion.

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[*] Philos. Trans. for 1810, p. 1

Part I. *On the Duration of Muscular Action.*

Duration of voluntary action.

The necessity of occasional intermissions from a series of laborious exertions is within the experience of every one; the fatigue of continuing the effort of any one voluntary muscle without intermiſſion even for a few minutes is also sufficiently known; but there is a third view of the duration of muscular action, which appears to have escaped the notice of physiologists; for I believe it has not hitherto been observed, that each effort, apparently single, consists in reality of a great number of contractions repeated at extremely short intervals: so short indeed, that the intermediate relaxation cannot be visible, unless prolonged beyond the usual limits by a state of partial or general debility.

Every action a series of repeated efforts.

Proof of this from the vibratory sound of a finger in the ear,

I have been led to infer the existence of these alternate motions from a sensation perceptible upon inserting the extremity of the finger into the ear. A sound is then perceived, which resembles most nearly that of carriages at a great distance passing rapidly over a pavement.

which does not arise from pressure on the tympanum.

The rapidity of the motion varies according to the degree of force, with which the finger is retained in its place. The sound thus perceived is not at all dependent on the degree of pressure upon the tympanum; for, on the contrary, the vibratory sound is most distinct, when this pressure is slight, if the finger be at the same time rendered rigid by the forcible action of antagonist muscles; and when the ear is stopped with great force without the presence of muscular action, no such sound is produced. For instance, if the head be rested upon the hand in such a position, as to press with its whole weight upon the ball of the thumb applied to the ear, no noise is perceived, unless the extremity of the thumb be at the same time pressed against the head, or unless the action of some other muscles be communicated to the ear, by any inadvertence in the method of conducting the experiment

When

Frequency of the vibrations.

When I endeavoured to estimate the frequency of these vibratory alternations, they appeared to be in general between 20 and 30 in a second; but it is possible, that the method I employed may be found defective; and it is to be hoped, that my estimate may be corrected, by some means better adapted to the determination of intervals, that cannot actually be measured.

It was by imitation alone, that I was enabled to judge of their frequency. For this purpose I contrived to render the vibration itself, and the imitative sound, both audible by the same ear.

Method of measuring this.

While my ear rested on the ball of my thumb, my elbow was supported by a board lying horizontally, in which were cut a number of notches of equal size, and about ¼ of an inch asunder. Then, by rubbing a pencil or other round piece of wood with a regular motion along the notches, I could imitate pretty correctly the tremor produced by the pressure of my thumb against my head, and by marks to indicate the number of notches passed over in 5 or 10 seconds, observed by my watch, I found repeated observations agree with each other as nearly as could be expected; for I could not depend upon exerting the same degree of force in different trials.

This experiment varied.

That I might not be deceived by the resemblance of tremors, which coincided only at alternate beats, and therefore might be considered as octaves in music, to each other, I sometimes employed notches at greater and sometimes at less distances from each other, but the result was nevertheless the same; and in order to avoid any errour that might be caused by some accidental quality of the sound arising from the length of the muscle employed, or length of the bones concerned in conveying the imitative sound to my ear, I made the following variation of the experiment. My ear was stopped by a cushion pressed upon by the end of a notched stick that rested on my foot, and thus conveyed the vibration from the muscles of my leg to the ear, along with the tremor produced by friction upon the notches; and still the results were nearly the same; varying in frequency between 20 and 30 in a second, according to the degree of force exerted in the experiment*.

As a farther proof, that I was not much deceived in my judgment of the frequency of these vibrations, I requested two or three of my friends to repeat the same experiment for me, and our agreement was such, as to confirm me in the opinion, that there could be no very considerable errour in the estimation.

Extremes of frequency.

The greatest frequency, that I think I have observed, was about 35 or 36 in a second, and the least was as low as 14 or 15; but in attempting to lessen the number of vibrations, there appears to be a degree of unsteadiness, which prevents any accurate measurement of the real number.

Trembling of age or weakness.

It is very probable, that in cases of great debility the number may be even considerably less, and may be the reason of that visible unsteadiness, which is known to occur in persons enfeebled by age, or much reduced by disease.

Possibly the foregoing observation may not be altogether new to some members of this Society, as it is now about 17 or 18 years since it first occurred to me, and I was then accustomed occasionally to mention it in conversation with my friends; but I am not aware that any other person has made the same remark respecting the vibratory nature of muscular action, although I find, that Grimaldi had observed the sound that occurs upon stopping the ears, but ascribed it, according to the notions that prevailed in his time, to the hurried motions of the animal spirits †.

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* The resemblance of the muscular vibrations to the sound of carriages at a distance I apprehend to arise not so much from the quality of the sound, as from an agreement in frequency with an average of the tremors usually produced by the number of stones in the regular pavement of London, passed over by carriages moving quickly.

If the number of vibrations be supposed 24 in a second, and the breadth of each stone be about 6 inches, the rate of a carriage thus estimated would be about 8 miles an hour, which agrees with the truth as nearly as the assumptions on which the estimate is founded.

† Vera utique ratio experimenti prædicti est, quia in digito et brachio [illegible] continuato fiunt multi motus ac tremores, ob spiritum anim[illegible] hinc illuc perpetuo accurrentium.

GRIMALDI, Physicomathesis de Lumine, p. 383.

PART II.

PART II. *On Seasickness.*

Seasickness.

The second remark which I have to offer to the society relates to seasickness, the cause of which has not hitherto been fully explained, and although the explanation which I am about to propose may not appear altogether satisfactory to persons, who, when at sea, are also rendered giddy by the incessant motion of the waves, and are consequently liable to consider as cause and effect phenomena which in their minds are constantly associated, yet the observation on which it is founded may deserve to be recorded, on account of the degree of relief that may be obtained in that most distressing affection.

Power of resisting its effects acquired

After I had been harassed by seasickness during a short voyage for some days, and had in vain attempted to account for the difference between the inexperienced passenger, and those around him more accustomed to the motion of the sea, I imperceptibly acquired some power of resisting its effects, and had the good fortune to observe a peculiarity in my mode of respiration, evidently connected with the motion of the vessel, but of which, in my then enfeebled state, I was unable to investigate either the cause or consequence. In waking from a state of very disturbed sleep, I found that my respirations were not taken with the accustomed uniformity, but were interrupted by irregular pauses, with an appearance of watching for some favourable opportunity for making the succeeding effort, and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion.

P[illegible] [illegible] noticed [illegible]

The mode by which I afterward conceived, that this action could primarily affect the system, was by its influence on the motion of the blood; for, at the same instant that the chest is dilated for the reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the egress of blood is so far obstructed, that, when the surface of the brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen in alternate motion with the different

which [illegible] circulation [illegible]

Headach

different states of the chest. It is probably from this cause, that, in severe headache, a degree of temporary relief is obtained by occasional complete inspirations.

The respiration will counteract the pressure of the blood on the brain,

In seasickness also the act of inspiration will have some tendency to relieve, if regulated so as to counteract any temporary pressure of blood upon the brain; but the cause of such pressure requires first to be investigated.

which is produced by the subsidence of the vessel,

All those who have ever suffered from seasickness (without being giddy) will agree, that the principal uneasiness is felt during the subsidence of the vessel by the sinking of the wave on which it rests. It is during this subsidence, that the blood has a tendency to press with unusual force upon the brain.

If a person be supposed standing erect upon deck, it is evident that the brain, which is uppermost, then sustains no pressure from the mere weight of the blood, and that the vessels of the feet and lower parts of the body must contract, with a force sufficient to resist the pressure of a column of blood of between five and six feet from the head downwards.

If the deck were by any means suddenly and entirely removed, the blood would be no longer supported by its vessels; but both would fall together with the same velocity by the free action of gravity; and the same contraction of the vessels which before supported the weight of the blood would now occasion it to press upon the brain, with a force proportional to its former altitude.

In the same manner, and for the same reason, during a more gradual subsidence of the deck, and partial removal of support, there must be a partial diminution of the pressure of the blood upon its vessels, and consequently, a partial reaction upon the brain, which would be directly counteracted by a full inspiration.

The consequence of external motion upon the blood will be best elucidated by what may be seen to occur in a column of mercury similarly circumstanced.

A barometer, when carried out to sea in a calm, rests at the same height at which it would stand on shore; but, when the ship falls by subsidence of the wave, the mercury is seen apparently to rise in the tube that contains it, because a portion of its gravity is then employed in accelerating its descent along with the vessel; and, accordingly, if it were confined in a tube closed at bottom, it would no longer press with its whole weight upon the lower end. In the same manner, and for the same reason, the blood no longer presses downwards with its whole weight, and will be driven upwards, by the elasticity which before was merely sufficient to support it.

Sickness from swinging.

The sickness occasioned by swinging is evidently from the same causes as seasickness; and that direction of the motion, which occasions the most piercing sensation of uneasiness, is conformable to the explanation above given.

It is in descending forward, that this sensation is perceived; for then the blood has the greatest tendency to move from the feet toward the head, since the line adjoining them is in the direction of the motion. But when, in the descent backwards, the motion is transverse to the line of the body, it occasions little comparative inconvenience, because the tendency to propel the blood toward the head is then inconsiderable.

Not fully prevented by inspiration.

The regularity of the motion in swinging afforded me an apparently favourable opportunity for trying the effect of inspiration; but although the advantage was manifest, I must confess, it did not fully equal the expectations I had formed from my experience at sea. It is possible, that the suddenness of the descent may in this case be too great, to be fully counteracted by such means; but I am inclined to think, that the contents of the intestines are also affected by the same cause as the blood; and if these have any direct disposition to regurgitate, this consequence will be in no degree counteracted by the process of respiration.

Contents of the intestines affected also.

Effect on the stomach in some measure counteracted.

A friend of mine informed me, that he had endeavoured to counteract this mechanical effect upon the stomach, and had experienced immediate relief from a slight degree of seasickness, by lying down upon the deck with his head towards the stem of the vessel; by means of which, upon pitching, he was in the attitude of a person descending backwards in a swing.

Affect on the stomach instantaneous.

Whether the stomach be or be not thus primarily affected, or only by sympathy with the brain, the sensation of sinking is in all cases referred directly to the stomach, which is seized with such instantaneous retching, that no person who has not been so situate, can form a just conception of it*.

Tendency to faint from the pressure on the brain being withdrawn.

In thus referring the sensations of seasickness, in so great a degree, to the agency of mere mechanical pressure, I feel confirmed by considering the consequence of an opposite motion, which, by too quickly withdrawing blood from the head, occasions a tendency to faint, or that approach to fainting, which amounts to a momentary giddiness with diminution of muscular power. At a time when I was much fatigued by exercise, I had occasion to run to some distance, and seat myself under a low wall for shelter from a very heavy shower. In arising suddenly from this position I was attacked with such a degree of giddiness, that I involuntarily dropped into my former posture, and was instantaneously relieved, by return of blood to the head, from every sensation of uneasiness.

Since that time, the same affection has frequently occurred to me in slighter degrees, and I have observed, that it has always been under similar circumstances of rising suddenly from an inclined position, after some degree of previous fatigue. Sinking down again immediately removes the giddiness; and then, by rising a second time more gradually, the same sensation is avoided.

Earthquakes affect the stomach.

* There is one occasion, upon which a slighter sensation of this kind is perceived, and it appears to indicate the direction of the motion from which it arises, to be downwards. "In a country subject to frequent returns of earthquakes," it is said * "a few minutes before any shock came, many people could foretel it by an alteration in their stomachs; an effect which" (it is added) "always accompanies the wavelike motion of earthquakes, when it is so weak as to be uncertainly distinguishable." (Michell, Phil. Trans. vol. LI[illegible]

It seems, that the vapours to which these tremendous concussions are owing, immense in quantity, and of prodigious force, being for a time confined on all sides, elevate the surface of a country to a vast extent, until they either find vent, or meet with some partial cause of condensation; and hence the alternate heaving and subsidence of the ground will produce much the same effects as the rising and falling of the swell at sea.

* Phil. Trans. vol. XLII, p. 41.

PART III. Of the salutary Effects of Riding, and other Modes of Gestation.

Gestation

In the preceding instances of disturbing [illegible]ulation of the blood by external motion, the effect [illegible] able, and proportionally prejudicial. There may [illegible] cases of disorder, in which it will be salutary; b[illegible] e probably less frequent, than is generally supposed.

should be distinguished from exercise in general;

In the observations which follow, general opinion will concur with me, on the benefit derived from exter[illegible]l or passive motion; and I hope, that, in ascribing its good effects to their true cause, I shall enable others [illegible] a valuable distinction, which has not yet been preserved with due care, between one motion which is salutary, and another which is very frequently pernicious. For, although the term *gestation* is employed by medical writers, as a general term comprehending riding on horseback, or in a carriage; and although the merits of such motions, especially the former, were clearly noticed, and perhaps even overrated, by the discernment of Sydenham: I believe, that no explanation has yet been given of the peculiar advantages of external motion, and am persuaded, that the benefits to be derived from carriage exercise are by no means in so high estimation as they ought to be.

active being injurious, where passive is salutary.

Under the common term *exercise*, active exertion has too frequently been confounded with passive gestation, and fatiguing efforts have consequently been substituted for motions that are agreeable, and even directly invigorating, when duly adapted to the strength of the invalide, and the peculiar nature of his indisposition.

Effects of gestation on the circulation of the blood,

The explanation, which I am about to offer, of the effects of [illegible]nal motion upon the circulation of the blood, is founded upon a part of the structure observable in the venous system, the mechanical tendency of which cannot be doubted. The valves, which are every where dispersed through those vessels, allow free passage to the blood, when propelled forward, by any motion that assists its progress; but they oppose an immediate obstacle to such as have a contrary tendency. The circulation is consequently helped forward by every degree of gentle agitation. The heart is supported in any laborious effort, that may have become necessary by some obstacle to its exertions; it is assisted in the great work of restoring a system, which has recently struggled with some violent attack; or it is allowed, as it were, free, to shun a labour, to which it is unequal, when the funds of life are nearly exhausted by any lingering disorder, which,

other vital functions,

In the relief it has afforded to an organ so essential to life, all other vital functions must necessarily participate; and the various offices of secretion, and assimilation, by whatever means they are performed, will not fail to be promoted during such comparative repose from laborious exertion.

and even the mind.

Even the powers of the mind itself, though apparently least likely to be influenced by mere mechanical means, are manifestly, and in many persons, affected most immediately by these kinds of motion.

Inability from fullness of blood.

It is not only in cases of absolute deficiency of power to carry on the customary circulation, that the beneficial effects of gestation are felt, but equally so, when comparative inability arises from redundancy of matter to be propelled. When from fulness of blood the circulation is obstructed, the whole system labours under a feeling of hurry and agitation, with that sensibility to sudden impressions, which is usually termed nervousness. The mind becomes incapable of any deliberate consideration, and is impressed with horrours, that have no foundation but in a distempered imagination.

Nervousness.

Influence of gestation in a carriage.

It is in moderate degrees of this species of affection, that the advantages of carriage exercise are most sensibly felt. The composed serenity of mind, that succeeds to the previous alarm, is described by some persons with a degree of satisfaction, that evinces the decided influence of the remedy. With this steadier tone of mind returns its full power of cool reflection; and if the imagination becomes more alive than usual, its activity is now employed in conceiving scenes, that are amusing and agreeable.

Striking instance of it.

As an instance of direct relief to a circulation labouring from mere fulness of blood, I may adduce that of a person, whose friends, as well as himself, were apprehensive, from the violent and visible throbbing of his heart, of the existence of some organic mischief, and were in some measure alarmed for the consequences.

He was persuaded, and not reluctantly, to go without delay for medical advice; and was accordingly [illegible] ved in a carriage to the house of some physician of [illegible] but did not succeed in finding him at home. As [illegible] was did not appear to admit of delay, and wer[illegible] not aggravated by the motion, it was hoped, that the wished for advice might be obtained at a part of the town which happened to be at some distance. But the second attempt proved as fruitless as the former, and a third was made with the same event. Since the throbbing had by that time considerably abated, he was contented to postpone any farther efforts to the following day, and directed the carriage homewards. By the time that he returned to his friends, he found, that the motion of travelling over several miles of pavement had apparently removed the complaint. The pulsation of the heart and arteries had subsided to their natural standard, and he congratulated himself, that his search of a remedy had not been ineffectual, although he had been disappointed as to the source, from which he thought he had most reason to expect relief.

Vigour directly given.

If vigour can in any instance be directly given, a man may certainly be said to receive it in the most direct mode, when the important service of impelling forward the circulation of his blood is performed for him by external means. The main spring, or first mover of the system, is thereby, as it were, wound up; and although the several subordinate operations of so complicated a machine cannot be regulated in detail by mere external agency, they must each be performed with greater freedom, in consequene of this general supply of power.

Benefit of gestation in chronical diseases.

In almost every treatise on the subject of chronical diseases are to be found numerous instances of the benefit, produced by the several modes of gestation, which have been most generally adopted; as riding on horseback, in carriages, sea voyages, and swinging. And in many cases, which might be adduced, it has appeared too clear, to admit of a doubt, that the cure of the patient has been owing [illegible] to the external agitation of his body, which must be allowed, at least, to have had the effect above explained: that of relieving the heart and arteries from a great part of their exertion in propelling the blood, and may [illegible] have contributed to the cure by this means ou[illegible]ree, to the[illegible]

It should be employed in different modes.

[illegible] by [illegible]t modes above mentioned are adapted from their [illegible] which different degrees of bodily strength; and if there are cases, in which that which appears most eligible may not suit the situation or circumstances of the patient, it can not be difficult to contrive other means of giving motion, so as least to incommode, and yet to give the greatest relief. "A very gentle and long continued, or even incessant motion, may suit some cases better than any more violent and occasional agitation; and in this way, probably, it is, that sea voyages have sometimes been attended with remarkable advantage.

Sea voyages.

VIII.

Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.*

(Concluded from p. 193.)

Observations respecting Trees of different Lengths in the Bole.

Increase of trees of different lengths of bole at different ages,

TREES that increase annually 12 inches in height and one in circumference, and have boles of different lengths, these boles, if of the undermentioned lengths, increase after the rate of 5 per cent per annum at the ages and heights under-mentioned, and they measure as under, viz,

									Contents		
			Years old		In.		Ft.		Ft.	in.	p
Trees with	12 feet	boles at	46	their girt	10	at	6	high,	8	4	0
Do	16	do.	49	do	10	at	8	do	1[illegible]	1	4
Do	21	do.	53	do	10	at	12	do	1[illegible]	[illegible]	0
Do.	32	do	56	do.	10	at	16	do	2[illegible]	2	[illegible]
Do	40	do.	60	do.	10	at	20	do	27	9	4
Do.	48	do.	64	do	10	at	24	do,	33	4	0

Whatever the lengths of the boles of trees increasing as above may be, the increase is 5 per cent per annum one year after their girt in the middle is 10 inches, but not longer.

But supposing that these trees have grown to [illegible] years of age, and increased as above-mentioned, then [illegible] contents at that age would be as under, viz. and at 60 years

							[illegible]s.		
								in.	p.
Trees with	16	ft. boles,	13	inches girt at	8	ft high	18	9	4
Do	20	do	12½	do.	10	do.	[illegible]	8	5
Do.	24	do	12	do.	12	do.	[illegible]	0	4
Do	32	do	11	do.	16	do.	[illegible]	10	8
Do.	40	do	10	do	20	do	27	9	4

This table shows, that the advantage to be gained by pruning trees higher than 32 feet is not an object worthy of consideration, if the trees are to be cut down at the age of 60 years. Boles of 32 feet,

And if it should be found, that, the higher a tree is pruned, the slower it swells in the bole, perhaps a 24 feet bole may measure as much at 60 years old as a 32 feet bole. If it increases half an inch in girt in the last 36 years more than the 32 feet bole increases in the same time, it will very nearly equal it in measure. 24 feet,

A 32 feet bole with a top from 20 to 30 feet high, with many large lateral branches, is certainly a much finer object than a forty feet bole with a top only twenty feet high, with a few and small lateral branches: and at sixty years old, the former will have had to increase in the last twenty-eight years only one quarter of an inch in girt, more than the latter, to exceed it in measure, to say nothing of the excess of timber in the larger top and branches. It must, however, be remarked, that at eighty years of age, the forty feet bole will exceed the thirty-two feet bole nearly six feet; and at one hundred years, thirteen feet, provided it swell equally fast in thickness. But unless the trees be oak, fit for the use of the navy, for which an increased price can be had, I imagine few gentlemen would now choose to let their trees stand to eighty years of age, when the increase of their boles will not be four per cent; still fewer would let them stand to one hundred, when the increase will not be three per cent per annum. and 40 fe

Again

Trees at 60 years of boles from 10 to 50 feet.

Again, let it be supposed, that trees sixty years of age have increased annually, during their growth, fifteen inches in height, and one inch and a half in circumference, the girt and contents of their boles, if of the under-mentioned lengths, will be as under, viz.

							Contents		
							Ft	in	p
Trees with	[illegible] ft	boles will be	19½ in.	girt at	10	ft high,	52	9	9
Do	[illegible]	do	18¾	do	12½	do	61	0	5
Do	30	do	18	do	15	do	67	6	0
Do	40	do	16½	do	20	do	75	7	6
Do	50	do	15	do	25	do	78	1	6

Taking it for granted, that the shorter boles will increase faster in thickness than the longer ones, it is reasonable to expect, that the forty feet bole will contain more timber than the fifty feet bole when they are both sixty years old, and if they are both sold at the same rate per foot, the forty feet bole must consequently be more valuable.

Long boles may be more valuable in some instances.

If, however a higher price can be had for longer boles, this may compensate not only for their deficiency in measure at sixty years of age, but also for their standing beyond the period when they cease paying the common rate of interest for the money they are worth, which I suppose is frequently the case as to tall elm trees, fit for keel pieces, and perhaps

Elm and beech.

beech for ship planking. It is hence evident, that, where the soil is such as will enable trees to grow to a great height, it will be necessary, before we decide how high to prune them, to consider to what purposes the timber can be most advantageously appropriated.

Whatever the lengths of the boles of trees increasing as above may be, their increase is five per cent per annum, one year after their girt in the middle is 15 inches, but not longer.

Trees at 60 years of boles from 24 to 60 feet.

Again, let it be supposed, that trees sixty years of age have increased annually, during their growth, eighteen inches in height, and two inches in circumference, the girt and contents of their boles, if of the undermentioned lengths, will be as under, viz.

							Contents		
							Ft	in	p
Trees with	24 ft	boles will be	26 inches	girt at	12	ft high,	112	8	0
Do	30	do	25	do	15	do	130	2	6
Do	36	do	24	do	18	do	144	0	0
Do	48	do	22	do	24	do	161	4	0
Do	60	do	20	do	30	do	166	8	0

Here again we may suppose, that the forty-eight feet bole, by swelling faster than the sixty feet bole, may exceed it in measure at sixty years of age; and this it would do, were the girt increased only half an inch. And if the thirty-six feet bole was increased two in [illegible], it would exceed both the forty-eight and s[illegible]les. But trees of such swift growth are frequ[illegible] down before they are sixty years old. At forty years of age the thirty-six feet bole, if it swell no faster than the forty-eight feet bole, will contain more timber if measured according to the present erroneous method. (The greater dispro-portion there is between the two ends of [illegible] of timber, the more disadvantageously it measures, when the girt is taken in the middle.) I suppose that in timber of this swift growth, the longer boles are frequently not worth more per foot than the shorter boles; therefore, in this case, that length of bole should be fixed on, which is likely to measure most at the period when the trees are intended to be felled.

Present method of measuring erroneous.

Whatever the lengths of the boles of trees increasing as above may be, their increase is five per cent per annum, one year after their girt in the middle is 20 inches, but not longer.

It appears from the last observations and calculations, that the annual increase in the boles of trees by their growth ceases to be equal to five per cent per annum some time between forty-six and sixty years of age, according as the boles are shorter or longer.

But it being generally allowed, that oak trees, of a size fit for the navy, require to grow from eighty to one hundred and fifty years, according to the quality of the soil; and it is so stated in the eleventh report of the commissioners appointed to inquire into the state and condition of the woods, forests, and land revenues of the crown; I have therefore been calculating tables, showing what the proportionably advanced prices should be, at different periods, up to one hundred and fifty years, to pay the proprietors for letting their trees stand to those periods. These prices, especially at the later periods, very greatly exceed any that have ever been given. It certainly has been much the

Size of oak for the navy require a very high price.

interest of the growers of oak timber to fell it at about sixty years of age, even if they replant the same ground. To let it stand to one hundred and twenty years of age, and sell at the present prices, their loss would exceed double, to the value of the timber at sixty years of age. No[illegible] by [illegible] of a sufficient price will long command a sufficient [illegible]. Owing to too low prices the quantity of large timber on private estates has long been rapidly decreasing; and it will be too late to commence offering reasonable prices for it, when it is all gone, and no oaks left of greater growth than sixty years. To have to wait then growing the second sixty years may bring upon us evils exceeding all calculation.

Loss on [illegible] it standing 120 years.

Valuations made in October, 1807, of several Plantations in Staffordshire

Instances of profit on plantations of oak,

The valuations were made of the trees growing within the space of a chain square, being the tenth part of an acre, of the medium growth of each plantation.

In the plantation by the mill wall there are now growing within twenty-two yards square, as under, viz.

	£.	s.	d.		£.	s.	d
70 oak trees, containing 175 feet, at 2s. 3d.	19	13	9				
1200 of oak bark, at 12s.	7	4	0				
	26	17	9	or, per acre,	268	17	6

The above is part of about four acres planted in 1775, on a strong loamy soil, worth about 20s. an acre.

	£.	s.	d.
One pound per ann. forborn 32 years, and improved at 5 per cent compound interest, would amount to	75	6	0

But the value of the timber is more than three times this amount.

The ground was prepared for planting by ploughing.

On the east side of Cottage Wood there are now growing, ash, within twenty-two yards square, as under, viz.

	£.	s.	d.		£.	s.	d.
50 ashes, containing 300 feet, at 1s. 6d. ······	22	10	0				
13 oaks do. 7 do. 2s.·····	0	14	0				
Bark ················	0	7	0				
	23	11	0	or, per acre,	235	10	0

The above is part of about two acres, planted in 1776, partly on heaps of earth in clay pits, and partly on strong soil upon a deep bed of sand, value about 15s. an acre.

	£.	s.	d.
Fifteen shillings per annum, forborn 31 years, and improved at 5 per cent compound interest would amount to	53	0	0

But the value of the timber is more than four times this amount.

In the clay pits only holes were dug for the plants, but the other part wholly trenched, or double dug with the spade.

In Pickmore Pool Plantation there are now growing, and fir. within twenty-two square yards, as under, viz.

	£.	s.	d.		£.	s.	d.
97 Scotch firs, containing 636 feet*, at 1s. . .	31	16	0	or, per acre,	318	0	0

The last plantation is part of about six acres planted in the springs of 1778 and 9. Much of the soil is a tough peat on gravel or hungry white sand, worth, say, 5s. per acre.

This ground lay between two tenants, who had never cultivated it. They had then nineteen years unexpired of their lease of thirty-one years of this and the adjoining lands, and willingly gave it up to be planted, on condition of having the fences made and kept in good repair.

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* This produce is after the rate of 6360 feet an acre, which is about the rate of Table IV.

	£.	s.	d.
Five shillings a year, forborn 29 years, and improved at 5 per cent compound interest, would amount to	15	11	0

But the value of the timber is more than twenty times this amount.

The trees were about two feet high, and planted at two yards distance, in holes dug with the spade, 1210 on an acre. Labour of making the holes and planting the trees cost 1l. 6s. 10½d. per acre.

About 2700 were planted on an acre in the other plantations, where the ground was wholly broken up.

Thinnings pay expenses.

In the remarks on these three plantations, no notice is taken of the thinnings. I am informed by gentlemen who have kept accounts of thinnings, that these have repaid the rent of the land, and every expense, with compound interest, some time before the woods were thirty years old; and the preceding calculations show, that it may be so. And if so, the present value of these plantations is all clear gain.

The valuer of these plantations has bought a good deal of wood out of them; and the prices he has valued at per foot may possibly be a fair value there for such small timber.

Firs on poor ground.

The growth of the firs in the last mentioned plantation is probably as great in that poor ground as it would have been had they been planted on ground of three or four times its value; this must be a powerful inducement to a gentleman to plant all such poor ground in the first instance.

Trees on farms advantageous in screens,

not in hedge-rows.

And a few of oaks, ashes, and firs may be raised on almost every farm in screens, that may, by their shelter, increase the value of the farm to the occupier, by increasing the produce, particularly that of grass grounds. In this case the interest of landlord and tenant may be reciprocal; but it is the reverse, where trees are planted in hedge-rows.

Beneficial on the tops and sides of mountains.

And even the sides and tops of high mountains may be made abundantly more productive of grass, if certain portions of them were surrounded by plantations. These plantations, by breaking the force of cold winds, diminish their chilling effect on the fields the plantations surround, and render the climate on mountains much more mild and genial.

This last kind of improvement will generally be found very greatly to exceed the expectation of the improver, provided it be judiciouſly planned and executed.

C. WAISTELL.

Additional Remarks[*].

Loss from not thinning plantations properly,

Great loss is frequently sustained by omitting to thin plantations properly, and in due time, but I am not in possession of facts, to calculate with accuracy what this loss may be; I will however venture to give a short statement of some calculations I have made, as to the loss that would now be sustained, by letting trees grow to a great age.

and from letting trees stand too long.

In Miller's Gardener's Dictionary it is stated, that, in a fall of oak timber in Lord Bagot's woods, Mr. Marshall counted the rings of one tree, which was sound at the butt, and found the number to be about 200. Its bole was 22 feet long, and 108 inches in circumference in the middle. Its contents 110 feet, which at 2*s*. amount to 11*l*. I think it was last year, that a fine sound oak tree was cut down, between Shrewsbury and Oswestry in Shropshire, of 300 years of age, and sold by auction for 52*l*. 5*s*.—And under my direction, many oak trees were cut down, some years ago, that could not be less than 300, and some of them probably 400 years of age, and even more. In Hunter's Evelyn's Sylva is given the circumference of 10 trees, not one of which was probably less than 500, and some of them probably 1000 years old.

Lord Bagot's tree of 200 years old, above-mentioned, would, at the present price of 3*s*. a foot, be worth 16*l*. 10*s*. Supposing that 3*s*. a foot should continue to be the price of oak timber, for the next 200 years, we will inquire what sum might be raised by growing four oak trees in succession, upon the same spot of ground, each tree to be cut down when 50 years of age, and that their boles

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[*] Trans. of the Soc. of Arts, vol. XXVII, p 81.

Loss from letting trees stand too long.

should be of the same length as that of Lord Bagot's, viz. 22 feet.

I fix on fifty years of age, as being convenient for my calculation; and nearly the most profitable period at which to cut down trees of 22 feet bole, which have grown at the medium rate of one inch in circumference, and 12 inches in height, annually.

After its 52nd year, *such a bole* ceases increasing after the rate of 5 per cent per annum*: but the whole tree, including the top part above the bole, may continue increasing after that rate until its 61st year†.

I do not fix on 50 years of age as being the most profitable age at which to cut down trees; probably 60 or 70 years of age would in some instances be preferable. Supposing an oak tree has increased, as above-mentioned, its bole of 22 feet would, at 50 years of age, measure 39 inches in circumference at the middle; and one fourth of this, namely 9¾ inches, squared and multiplied into 22 feet, its length, gives 14 feet 6 inches for its contents, which at 3*s*. a foot, its present value, amounts to £2. 3*s*. 6*d*. Supposing £2 3*s*. 6*d*. to be the value of each of the four trees of fifty years of age, grown in succession upon the same spot of ground, in the period of 200 years, we will calculate to what the first three trees would amount, if their value was placed out at compound interest, for the respective terms of 150, of 100, and of 50 years.

£.	*s*.	*d*.		£	*s*.	*d*.
2	3	6	Accumulating during 150 years, at 5 per cent per annum compound interest, will amount to ········	3280	0	0
2	3	6	Accumulating as above for 100 years would amount to ········	286	0	0
2	3	6	Accumulating as above for 50 years would amount to ···············	24	0	0
			Add to the value of the tree to be cut down at the end of 200 years..	2	3	6
			Total amount in 200 years ········	3592	3	6

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* See Table 10 of a bole of 24 feet, p. 19.

† See my first Table, p. 28.

And carrying forward this calculation, the total amount of the produce in 300 years would amount to......................£47240[illegible]0

Oaks formerly valued for their acorns.

In former times, when the value of oak-woods were estimated by the number of hogs their acorns would fatten, the great age of trees would be of small consideration; but in the present times, I am persuaded, that, if gentlemen, who have many trees standing of the age of 150 years and upwards, would give this subject its due consideration, they will be aware of the immense loss, to which they are voluntarily subjecting themselves—And this great loss is much to be regretted, in a political point of view, especially as the produce of this island is insufficient for its necessary consumption.

IX.

Remarks on Professor WOOD'S *new Theory of the Diurnal Motion of the Earth round its Axis. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

Professor Wood's hypothesis not yet noticed.

I HAVE waited some time in hopes, that Prof. Wood's hypothesis, intended to subvert the Newtonian theory of the tides, would be noticed by some one or other of the able mathematicians in our country. They can scarcely think the production of the American professor so absurd, as to merit no attention: and on the other hand, if its truth be evident, it surely does not deserve to be passed over in silence. Nothing however having yet been said about it in your Journal, I am tempted to ask, whether the prof. have not led himself into an errour by confounding together absolute motion and relative motion, and reasoning on the effects of one from the quantity of the other.

Apparent source of his mistake.

That

The centrifugal force not increased in any point of an epicycloid

That the point of the circle generating an epicycloid, which is farthest from the centre of the circle round which it revolves, moves faster through space, and consequently has a greater absolute velocity, than the opposite point, I am not inclined to dispute. But I presume the motion, that produces centrifugal force, is the rotary motion round that centre, from which the particles of matter have a tendency to fly off: and it appears to me, that, the rotary motion of every point in the circumference of the generating circle round the centre of that circle being the same, its velocity with respect to that centre is uniform, and of course there is no alternate increase and diminution of this velocity, which produces the centrifugal force.

Farther illustration of this.

This is perhaps the simplest mode of considering the subject: but we might take it in another point of view. If we suppose the circle A D B E, in prof. Wood's diagram, vol. XXVI, pl. V, fig. 7, to be moving through space in the direction C C, while revolving in the direction A D B; and put *a* for the velocity with which A revolves round C, *b* for the velocity with which B revolves round C, and *c* for the velocity with which the centre, C, is carried in the direction C C: then, as the motions of A and C are in the same direction, the absolute velocity of A through space will be $a+c$; and, as the motions of B and C are in opposite directions, the absolute velocity of B will be $b-c$. But the relative velocity of A with respect to C will be simply *a*; because from its absolute velocity, $a+c$, we must deduct the velocity of C, moving in the same direction, and $a+c-c$ obviously $=a$: and again, the relative velocity of B with respect to the centre C must continue $=b$; because, if to its absolute velocity, $b-c$, we add that of C, moving in the opposite direction, we shall have $b-c+c=b$. Now *a* and *b* are clearly equal, because they merely express the velocity, with which two different points in the circumference of the same circle revolve round its centre: and therefore the centrifugal force is not in any way affected by the epicycloidal motion.

I am, Sir,
Your very obedient humble servant,

Nov. 12, 1810. T. NOOT.

Remarks

REMARKS.

The hypothesis of prof. Wood I believe is not new. I understand the same theory suggested itself to Mr. James Ferguson, who valued himself upon the discovery; but his manuscript was never sent to the press, as, on showing it to some friends, it was thought to be founded on erroneous principles. The preceding letter I insert with pleasure, as it appears to me, to give a simple and satisfactory account of the subject: but, though such is my private opinion, I considered it proper to lay prof. Wood's circular letter before my readers; particularly as I knew that some, who do not rank among the most contemptible mathematicians, had not been able to make up their minds respecting it.

The hypothesis not new,

but not decided on.

X.

An analytical Essay on the Scammonies of Aleppo and Smyrna, with some Observations on the reddening of Litmus by Resins: by Messrs. BOUILLON-LAGRANGE *and* VOGEL*.

THE two sorts of scammony are obtained from the root of a plant, that grows in Syria. The finest and purest scammony is procured by making an incision in the root, and drying in the sun the juice that exudes. But frequently, in order to obtain a larger quantity, the people of Syria and Natolia express the juice, and not only from the root, but from the stalks and leaves also. Often too they adulterate it, by mixing with it the juice of some other milky and acrid plants, as that of the spurges; or increase its weight by a mixture of ashes and other foreign matters. To know that the scammony contains none of these heterogeneous substances, the buyer should break the lumps, choose those that are shining interiorly, and reject those that appear too black, burnt, or containing sand.

Two sorts of scammony how obtained.

Adulterations.

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* Annales de Chimie, vol. LXXII, p. 69.

Aleppo

Difference in their appearance.

Aleppo scammony is light, of an ashen gray, shining, transparent in its fracture. That of Smyrna is very compact, heavy, of a darker colour, and more difficult to powder.

Aleppo scammony exposed to heat.

Examination of Aleppo scammony.

When this scammony is pure, it melts entirely on a heated plate of iron, and emits fumes of a nauseating smell.

Treated with water,

Triturated with water it renders it milky. With boiling water it concretes into a lump: the water becomes yellow, and has a bitter taste; but it is neither alkaline, nor acid; which proves, that this substance is not adulterated with ashes, as some authors affirm.

Alcohol at 40° [sp. grav. 0·817] produces a slight precipitate in this aqueous solution; and the acetate of lead occasions a yellow flocculent precipitate soluble in nitric acid.

and with alcohol.

The alcoholic tincture of scammony has a brownish yellow colour. It reddens tincture of litmus; and leaves on evaporation a yellowish white and transparent resin.

Resin.

This resin dissolves entirely in nitric acid, which it colours yellow. The addition of water renders this solution slightly turbid.

Combines with potash,

and then with muriatic acid.

It is equally soluble in a solution of pure potash, even without heat, when its colour is yellow; but, if heat be employed, it is brown. Water, even in pretty large quantity, does not precipitate any of the resin. If the solution be saturated with muriatic acid, the resin does not separate. This triple compound of resin, acid, and potash, claims the notice of practitioners: perhaps we may thus discover a solvent for resins, that water would not render turbid.

Insoluble part.

The part of the scammony that was not soluble in alcohol assumed a gray colour when dry. Treated with boiling water it coloured it yellow, and alcohol occasioned a white flocculent precipitate in it.

Experiments to determine the proportions of its component parts.

To determine the proportion of the constituent principles of Aleppo scammony, we took 100 parts of this substance, and exhausted them by alcohol. The solution was yellow. A gray substance remained, which, when dried, weighed 26.

The alcoholic solution was evaporated to a sirupy consistence. Cold water precipitated from it a resin, which formed a homogeneous mass. The supernatant liquor was clear and colourless. Evaporated to dryness a brown matter was obtained, soluble both in water and in alcohol, and precipitable by acetate of lead. This substance appeared to be what is called extract. When dried it weighed 2 parts.

The resinous mass, separated and dried, was yellow, and weighed 60.

The 26 parts insoluble in alcohol were then treated with boiling water. After evaporation a glutinous matter remained, weighing 3 parts, and having all the characters of gum. The remainder consisted of fibres of vegetables and a little silex.

Subjected to dry distillation.

The distillation of Aleppo scammony exhibited nothing remarkable. Its products were a very acid brown liquor, and a light, blackish oil. The coal was black, shining, and compact. It contained the carbonates of potash and lime, alumine, silex, and a little iron.

Examination of Smyrna scammony.

Smyrna scammony less fusible.

Treated with water,

The fusion of Smyrna scammony is less complete than that of Aleppo. Instead of concreting into a lump with boiling water, it becomes clotty; but the water poured off has similar qualities.

and with alcohol.

An equal portion of this scammony, exhausted by boiling alcohol, afforded a tincture of a deeper colour, though containing less resin. By evaporation a brownish, transparent resin was obtained, weighing 28 parts. The matter insoluble in alcohol weighed 66. This residuum, treated with boiling water, coloured it yellow. The solution had a faint sweetish taste; and alcohol produced in it a flocculent precipitate soluble in water. On evaporation it left a thick glutinous matter resembling mucilage, soluble with heat in weak nitric acid, and letting fall on cooling a white pulverulent substance, that had all the characters of mucous acid.

Residuum boiled in water.

Insoluble parts.

In this experiment water took up only 8 parts of the matter insoluble in alcohol. The remainder was subjected to the action of nitric acid assisted by heat, which dissolved it with effervescence. Ammonia added to this solution threw down a precipitate soluble in potash. Potash and oxalate of ammonia too occasioned a precipitate. This residuum therefore, beside vegetable fibres and the substance insoluble in water and alcohol, which appeared to be oxigenized extract, was composed of alumine and carbonate of lime.

This substance, being incinerated, left a whitish powder, soluble in great part with effervescence in muriatic acid. This solution contained alumine, lime, and a little iron. The portion not soluble in muriatic acid, being treated with potash, yielded a siliceous precipitate on the addition of an acid.

Extractive matter.

The water employed to precipitate the resin left after evaporation a brown matter, weighing 5 parts, of a bitter taste, attracting the moisture of the atmosphere, soluble in alcohol, and copiously precipitated from its aqueous solution by acetate of lead. This substance exhibited all the properties of extract.

Component parts of Aleppo scammony,

From this analytical essay therefore it follows, that Aleppo scammony is composed of

Resin	60
Gum	3
Extract	2
Vegetable fibres, earthy matter, &c.	35
	100

and of Smyrna scammony.

and that Smyrna scammony contains

Resin	29
Gum	8
Extract	5
Vegetable fibres, &c.	58
	100

The resins have both apparently the same virtues.

Though the resins obtained from the two sorts of scammony have considerable analogy, yet, as that of the Aleppo is yellow, transparent, and friable, while that of the Smyrna is darker coloured, and more difficult to powder, we thought it would not be useless to ascertain, whether there were any difference in their medicinal properties. In consequence several physicians undertook to make comparative trials on persons of nearly similar constitutions, but they have not yet observed any difference in their purgative effects.

A gum-resin mixed with extract.

From the preceding analysis we may conclude, that scammony is a true gum-resin mingled with a little extract. It is true it contains much less gum than the other gum-resins, yet enough to form a milky liquor with water.

Litmus reddened by resins.

The action of the alcoholic tincture of scammony on litmus led us naturally to examine, whether the property of reddening this blue colour were owing to an acid. None of our experiments having furnished a direct proof of this, we made a comparative trial of some resins, which we subjected to the following experiments.

Gum sandarach.

1. *Sandarach.* This resin is converted into a grumous mass by boiling in water. The filtered liquid remains clear; and, when evaporated to a certain point, slightly reddens tincture of litmus; its taste is bitter; it does not alter infusion of violets; it is not precipitated by alcohol, or acetate of lead, which shows, that it contains neither gum nor extract. It is therefore a pure resin.

The resin thus treated with boiling water was dissolved in alcohol. This tincture strongly reddened that of litmus, and had no action on sirup of violets.

Powdered sandarach was digested in alcohol, and boiling water poured into the hot filtered solution, which precipitated the resin. The filtered liquid grew turbid on cooling; it had the strong smell of resin of sandarach; its taste was bitter; and its action on the tincture of litmus was so weak, that it could not be supposed to contain a free acid.

Mastic.

2. *Mastic.* This substance exhibits nearly the same phenomena as the preceding; but the resin concretes into a mass in boiling, like turpentine. The water has a bitter taste, and has no action either on litmus or sirup of violets. The resin, on the contrary, ſtrongly reddens tincture of litmus.

Olibanum.

3. *Olibanum* forms with hot water a thick pap, which is separated from the liquid with difficulty even by filtration.

This water has a blackish brown colour, is not precipitated by acetate of lead, and does not alter the colour of litmus; but alcohol throws down a copious precipitate from it, which proves, that this substance is composed of gum and resin.

The alcoholic tincture strongly reddens that of litmus.

If the resins that have most action on the colour of litmus be heated with all due precautions on a sand bath, no acid sublimes.

Treated with lime, according to Scheele's process, no calcareous benzoates are formed.

Various other resinous substances.

4. Lastly, the gum resin ammoniacum, myrrh, elemi, anime, galbanum, tacamahacca, resin of jalap, both prepared by ourselves and that of the shops, Venice turpentine, oil of turpentine, and several other resinous and gum-resinous substances, afforded the same results as those obtained from scammony, sandarach, and olibanum. From these facts it appears still difficult to solve the question, whether the reddening of litmus by resins be owing to the presence of an acid in them.

No proof that this is occasioned by an acid.

If acids alone had the property of reddening vegetable blues, we should not hesitate to admit their existence in resins, though not yet otherwise demonstrated by experiment. As to the infusion of violets not being reddened by resins, this property occurs in the sublimed acid of benzoin, which strongly reddens infusion of litmus, but does not alter the colour of violets. Has this acid, notwithstanding its solubility in water, some analogy to resins? On this we refrain from giving a decided opinion; yet we are inclined to believe, that this substance is a compound of a vegetable acid and a small portion of resin, to which perhaps its solidity is owing. Finally, as all the vegetable acids are soluble in water, it is difficult to ascribe to an acid this property in resins of reddening litmus. It appears more proper therefore, to consider the reddening of litmus as a character of resins, till fresh experiments have proved the contrary.

Benzoic acid perhaps combined with some resin.

Probably it is a character of resins to redden litmus.

SCIENTIFIC NEWS.

The Elements of Experimental Chemistry, by WILLIAM HENRY, *M. D. F. R. S. &c. The 6th Edition, greatly enlarged; and illustrated with Nine Plates engraved by* LOWRY. 2 *vols.* 8*vo.* 1138 *p.*

Henry's Elements of Chemistry.

FEW of our chemical readers can be unacquainted with the useful work of Dr. H., that first appeared in 1800, under the title of an Epitome of Chemistry, forming a volume of about 200 p. in 12mo; and subsequently increased, through successive editions, to a thick 8vo. The unexampled progress made in the science of chemistry within the last two years has rendered such an addition of new matter necessary, that the bulk of the present edition is more than double that of the last; and as much alteration was requisite on the same account in what had before been written, it may now be considered almost as a new work. Accordingly the author has altered its title, to render it more appropriate to its present state. It would be superfluous to say more of its merits, than that it is worthy of its author; and of a work so long before the public nothing more can be necessary, than to point out what has been done in the present edition. The chapters on Chemical Affinity, and Heat or Caloric, have received copious introductions, explaining their theory and laws. After the chapter on Water follows one entirely new on the Chemical Agencies of Common and Galvanic Electricity. The analysis of the fixed alkalis and ammonia, and an account of their bases, render the chapter on Alkalis almost wholly new; and nearly the same may be said of the following chapter on Earths. The chapter on Acids, which was a mere enumeration of their characteristic qualities in less than a page, now very advantageously occupies seven with a disquisition on their nature and properties. The introduction to chap. viii, on the General Properties of Metals, is completely rewritten, and much enlarged. Bitumens, and the Vegetable Principles of Asparagus, Elm tree gum, and Elecampane, form two new sections in addition to chap. xix. The introduction to chap. xxi, on Animal Substances; the sections on Gelatine, Albumen, and Mucus; and chap. xxiv, of the more complex Animal Products, may be considered as new. Indeed every chapter in Part I has received very considerable additions, particularly that on Vegetable substances; and scarely a page will be found without some alteration or amendment. In the 2d Part of the work, the Analysis of Minerals and Mineral Waters, and in the 3d, the Application of Tests to various useful Purposes, much less was to be done; yet these have not been left unimproved, particularly in one important point, the detection of arsenic in persons supposed to have been poisoned. In an Appendix are given all the discoveries in chemistry, that occurred during the progress of the work through the press, even up to those of Mr. Davy, that are to appear in the 2d part of the Phil. Trans. for the present year. Five or six new tables have been added to the useful collection in the preceding edition, two or three improved ones have been substituted for some of the former, and a few necessary corrections have been made in others. An additional plate too is given, so that the series now comprehends every article of apparatus essential to the pursuit of experimental chemistry.

Treatise on the doctrine of fluxions applied to military and naval science.

Mr. W. Moore, of the Royal Military Academy, will complete in the year 1811 a Treatise on the Doctrine of Fluxions, with its Application to all the most useful parts of the True Theory of Gunnery; The Motion of Rockets, in different Mediums; the Blowing up of Bridges, Fortifications, &c.; and several other new and important matters connected with Military and Naval Science. The Fluxions will be treated in the most easy manner, that the subject will admit; and the same correct principles observed throughout the performance; a thing which no Author in the English Language, that I am acquainted with, has done. The whole will be printed in 1 Vol. 8vo, and will be particularly adapted for all Military Institutions of eminence.

In

Permanent green said to be discovered in France,

In a late report made to the French Emperor on the arts and manufactures of France it is stated, that a grand improvement has been made in calico printing by the discovery of a permanent green. The English are represented as having offered great rewards and sought in vain for this process, which France has had the honour of discovering, and the advantage of which she alone will enjoy. It is now nearly two years since Mr. Ilett of Stratford took out a patent for a mode of producing permanent greens on linen and cotton by a process in all probability the same as that alluded to in the French report; for the English journals, in which Mr. Ilett's specification was given, must have made his process known in France. Shortly after the introduction of Mr. Ilett's colour, we understand a Mr. Thomson, a calico printer in Lancashire, discovered a mode of producing a permanent green on linen and cotton at one application, by a process entirely different from that of Mr. Ilett, in as much as it is compatible with all the various systems of colours afforded both by madder and weld. This last process supplies a great desideratum in the art of calico printing, and is capable of extensive application.

probably of English origin.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For NOVEMBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the Strand, London.

OCT. Day of	THERMOMETER.				BAROMETER, 9 A. M.	RAIN.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.			Day.	Night.
28	47°	43°	50°	35°	29·68		Rain	Fair
29	39	36	42	31	·63	·365	Fair	Ditto*
30	38·5	37	42	29·5	·83		Ditto	Ditto
31	35	44·5	44·5	40	30·02		Fair	Cloudy
NOV.								
1	44·5	40·5	47	36	29·74		Rain	Ditto
2	40·5	41	46	38	·79	·045	Ditto	Fair
3	44	44	46	40	·98	·045	Ditto	Cloudy
4	44	41	45	32	·90	·275	Ditto	Fair
5	35·5	39	40·5	36	·35†	·040	Fair[a]	Ditto[a]
6	40	40·5	43·5	36	·19	·030	Rain	Fair
7	41	41	42	36·5	28·95	·160	Ditto	Cloudy
8	41·5	42·5	46	35	29·10	·155	Ditto	Ditto
9	39	43	47	40	·48	·215	Fair	Foggy‡
10	46	43·5	48	39·5	28·80§	·540	Rain	Fair
11	45	44·5	46	40·5	29·24	·390	Ditto	Rain
12	44	40·5	45	35	·58	·170	Fair	Fair
13	37·5	39·5	44	35	30·13		Ditto	Ditto
14	39	40	42	40	·11		Rain	Rain
15	51·5	53	54·5	48·5	29·50	·595	Fair	Ditto
16	51	53	54·5	45	·28	·325	Ditto	Rain‖
17	48	47	51	42·5	·35	·070	Ditto	Fair¶
18	45	46	48	45	·54	·025	Fair	Rain
19	47	47	50·5	42	·59	·165	Rain	Cloudy
20	44·5	45·5	50	45	·67	·040	Cloudy	Rain
21	51·5	52	54	45	·43	·250	Rain	Fair
22	48	47	48·5	43	·54	·080	Ditto	Ditto
23	47·5	48·5	50	44	·84	·290	Fair	Rain
24	48	48·5	51	41	·75	·160	Rain	Ditto
25	45	45	50·5	39	·60	·320	Ditto	Fair
26	44	44	46·5	39	·32	·110	Ditto	Rain**

5·280 Inch. noted at 9 A.M.
for the preceding 24 hours.

* Snow in the night. † Uncertain. [a] Intervening fogs. ‡ Stormy night with rain.
§ Barometer at noon 28·61.
‖ Very fine at 14. ¶ Rain at 6 P.M. after fine. ** Evening fair, after 10 rain.

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXVII.

ARTICLE I.

Researches on the Oximuriatic Acid, its Nature and Combinations; and on the Elements of the Muriatic Acid: With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution. By H. Davy, *Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.*[*]

Discovery oximuriat acid.

THE illustrious discoverer of the oximuriatic acid considered it as muriatic acid freed from hidrogen [†]; and the common muriatic acid as a compound of hidrogen and oximuriatic acid; and on this theory he denominated oximuriatic acid dephlogisticated muriatic acid.

Mr. Berthollet [‡], a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxigen; and this idea for nearly twenty years has been almost universally adopted.

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[*] Philos. Trans. for 1810, p. 231. Communicated to the Royal Society at the request of the managers of the Royal Institution.

[†] Mem. Acad. Stockholm for 1774, p. 94.

[‡] Journal de Physique, 1785, p. 325.

but found, to my great surprise, that it was not at all volatile or decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance, that a substance composed principally of oximuriatic acid and ammonia should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

Properties of this compound.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were combustion, and the action of ignited hidrat of potash. When brought into the flame of a spirit lamp, and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow colour, and left a fixed acid, having the properties of phosphoric acid. When acted on by red-hot hidrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hidrat of potash.

Ammonia does not decompose phosphuretted or sulphuretted muriatic acid, but forms new compounds with them.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of Messrs. Gay-Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thompson; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat, but which seemed no farther decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of colour, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia. But as these compounds did not present the same uniform and interesting

ing properties, as that from the phosphoric sublimate, I did not examine them minutely: I contented myself with ascertaining, that no substance known to contain oxigen could be procured from oximuriatic acid, in this mode of operation.

Oximuriatic acid and ammonia form no water but dry muriat of ammonia and nitrogen.

It has been said, and taken for granted by many chemists, that, when oximuriatic acid and ammonia act upon each other, water is formed. I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oximuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gasses, and from 5 to 6 parts of nitrogen are produced; and the result is dry muriate of ammonia.

Action of oximuriatic gas and hidrogen.

Mr. Cruickshank has shown, that oximuriatic acid and hidrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water*; and Messrs. Gay-Lussac and Thenard have stated, that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oximuriatic acid gas, and hidrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oximuriatic acid and the hidrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though in proportion as the gasses were free from oxigen or water, this condensation diminished.

Oximuriatic acid gas and sulphuretted hidrogen.

I mixed together sulphuretted hidrogen in a high degree of purity and oximuriatic acid gas both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oximuriatic acid, was formed on the sides of the vessel; no vapour was deposited; and the residual gas contained about $\frac{19}{20}$ of muriatic acid gas, and the remainder was inflammable.

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* Journal, 4to series, vol. v, p. 201 and foll.

Existence of water in muriatic acid gas questionable.

Messrs. Gay-Lussac and Thenard have proved by a copious collection of instances, that, in the usual cases where oxigen is procured from oximuriatic acid, water is always present, and muriatic acid gas is formed; now, as it is shown, that oximuriatic gas is converted into muriatic acid gas by combining with hidrogen, it is scarcely possible to avoid the conclusion, that the oxigen is derived from the decomposition of water, and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxigen in oximuriatic acid gas.

Supposed proof of it doubtful.

Messrs. Gay-Lussac and Thenard indeed have stated an experiment, which they consider as proving, that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious, that in this case they formed the same compound as that produced by the action of oximuriatic acid on lead; and in this process the muriatic acid must loose its hidrogen, and the lead its oxigen; which of course would form water; these able chemists, indeed, from the conclusion of their memoir, seem aware, that such an explanation may be given, for they say that the oximuriatic acid may be considered as a simple body.

Action, on muriatic acid gas, of mercury,

I have repeated those experiments, which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find, that, when mercury is made to act upon 1 in volume of muriatic acid gas, by voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hidrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hidrogen is always from 9 to 11, the volume of the muriatic acid gas used being 20.

tin, and zinc.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas by heated tin and zinc, hidrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oximuriatic gas, resulted.

Scheele's hypothesis more just than the French.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oximuriatic and muriatic acids may be considered as an expression of facts; while the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

Oximuriatic acid and hidrogen form muriatic acid; and *vice versa*.

When oximuriatic acid is acted upon by nearly an equal volume of hidrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oximuriatic acid is attracted from the hidrogen, by the stronger affinity of the metal; and an oximuriate, exactly similar to that formed by combustion, is produced.

The muriates are compounds of oximuriatic acid with inflammable bases.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oximuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxigen may be conceived to be supplied to the tin, and hidrogen to the oximuriatic acid.

Oximuriatic acid and phosphorus.

The compound formed by burning phosphorus in oximuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxigen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hidrogen to the oximuriatic acid to form common muriatic acid gas.

Difference between muriates and oximuriatic combinations.

None of the combinations of the oximuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oximuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if Mr. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oximuriatic acid with potassium. Muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, it is decompounded; the oximuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hidrogen are set free.

Heat and light results of intense agency of combination solely.

The vivid combustion of bodies in oximuriatic acid gas, at first view, appears a reason why oxigen should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids become ignited during their mutual agency; and such an effect might be expected in an operation so rapid as that of oximuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oximuriatic acid consists of an acid basis united to oxigen, exists in the general analogy of the compounds of oximuriatic acid and metals to the common neutral salts. But this analogy, when strictly investigated, will be found to be very indistinct; and, even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water; and the metals of bases with hidrogen; and that in the case of the action of oximuriatic acid and metals, the metal furnishes hidrogen to form muriatic acid, and a basis to produce the neutral combination.

New view of the composition of neutral salts.

Quantity of hidrogen evolved from muriatic acid by metals no proof of the presence of water.

That the quantity of hidrogen evolved during the decomposition of muriatic acid gas by metals is the same, that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hidrogen with oximuriatic acid, one quantity must always be separated. Hidrogen is disengaged from its oximuriatic combination by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hidrogen is that which seems to adhere to oximuriatic acid with the least force.

I have

Electricity does not decompose oximuriatic gas,

I have caused strong explosions from an electrical jar, to pass through oximuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

or the oximuriates of phosphorus and sulphur.

I electrized the oximuriates of phosphorus and sulphur for some hours, by the power of the voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hidrogen, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hidrogen from Libavius's liquor by a similar operation; but I have ascertained, that this was owing to the decomposition of water, adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

What is the nature of the hyperoximuriates?

As there are no experimental evidences of the existence of oxigen in oximuriatic acid gas, a natural question arises, concerning the nature of those compounds, in which the muriatic acid has been supposed to exist combined with much more oxigen than oximuriatic acid, in the state in which it has been named by Mr. Chenevix hyperoxigenized muriatic acid.

Does oximuriatic acid combine with both oxigen and hidrogen? or is hyperoximuriatic acid the base of it and the muriatic?

Can the oximuriatic acid combine either with oxigen or hidrogen, and form with each of them an acid compound; of which that with hidrogen has the strongest, and that with oxigen the weakest affinity for bases? for the able chemist, to whom I have just referred, conceives, that hyperoximuriates are decomposed by muriatic acid. Or, is hyperoximuriatic acid the basis of all this class of bodies, the most simple form of this species of matter?

The phenomena of the composition and decomposition of the hyperoximuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

Unsuccessful attempts to obtain hyperoximuriatic acid separate.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxigenised, from hyperoximuriate of potash, by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a little oximuriatic acid is generated; yet oxigen is the chief gaseous product, and a muriate of potash not decomposable is produced:

The distillation of the orange coloured fluid, produced by dissolving hyperoximuriate of potash in sulphuric acid, affords only oxigen in great excess, and oximuriatic acid.

Facts unfavourable to the supposition of its existence.

When solutions of muriates, or muriatic acid, are electrized in the voltaic circuit, oximuriatic acid is evolved at the positive surface, and hidrogen at the negative surface. When a solution of oximuriatic acid in water is electrized, oximuriatic acid and oxigen appear * at the positive surface, and hidrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxigenised muriatic acid, whether it be imagined a compound of oximuriatic acid with oxigen, or the basis of oximuriatic acid.

Hyperoximuriate of potash probably contains potassium more oxided than in potash.

If the facts respecting the hyperoximuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oximuriatic acid, potassium, and oxigen. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxigen combined with the potassium; which we know has an intense affinity for oxigen, and which, from some experiments, I am inclined to believe, is capable of combining directly with more oxigen than exists in potash; than with the oximuriatic acid, which, as far as is known, has no affinity for that substance.

Decomposition of hyperoximuriate of potash by muriatic acid.

It is generally supposed, that a mixture of oximuriatic acid and hyperoximuriatic acid is disengaged, when hyperoximuriate of potash is decomposed by common muriatic acid †; but I am satisfied from several trials, that the gas procured

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* The quantity of oximuriatic acid in the aqueous solution is so small, that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

† If hyperoximuriate of potash be decomposed by nitric or sulphuric acid, it affords oximuriatic acid and oxigen. If it be acted upon

procured in this way, when not mixed with oxigen, unites to the same quantity of hidrogen*, as common oximuriatic acid gas from manganese; and I find, by a careful examination, that the gas disengaged during the solution of platina in a mixture of nitric and muriatic acids, which has been regarded as hyperoximuriatic acid, but which I stated some years ago to possess the properties of oximuriatic acid gas†, is actually this body, owing its peculiar colour to a small quantity of nitromuriatic vapour suspended in it, and from which it is easily freed by washing.

Oximuriatic acid not an acid,

Few substances, perhaps, have less claim to be considered as acid, than oximuriatic acid. As yet we have no right to say that it has been decompounded; and, as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxigen.

but possibly classing with oxigen as an acidifying principle

May it not in fact be a peculiar acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxigen, or oxides, in their properties and powers of combination; but differing from them, in being for the most part decomposable by water? On this idea muriatic acid may be considered as having

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upon by muriatic acid, it affords a large quantity of oximuriatic acid gas only. In this last case, the phenomenon seems merely to depend upon the decomposition of the muriatic acid gas by the oxigen loosely combined in the salt.

[*] This likewise appears from Mr. Cruickshank's experiments. See Nicholson's Journal, Vol. V, 4to, p 206.

The platina has no share in producing this gas.

[†] The platina, I find by several experiments, made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hidrogen of the muriatic acid attracts oxigen from the nitric acid. Oximuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red colour. Nitrous acid and muriatic acid produce no oximuriatic acid gas. Platina, during its solution in perfectly formed aqua regia, gives only nitrous gas and nitrous vapour; and I find, that rather more oximuriatic acid gas is produced, by heating together equal quantities of nitrid acid of 1·45, and muriatic acid of 1·18, when they are not in contact with platina, than when exposed to that metal. The oximuriatic acid gas, produced from muriatic acid by nitric acid, I find combines with about an equal volume of hidrogen by detonation.

hidrogen for its basis, and oximuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oximuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oximuriatic acid, may be regarded as analogous bodies. The combinations of oximuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

Chemical nomenclature requires a material change.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition, and in a more advanced state of the inquiry it will be necessary for the progress of science, that it should undergo material alterations.

Compounds of oximuriatic acid with inflammable substances.

It is extremely probable, that there are many combinations of the oximuriatic acid with inflammable bodies, which have not yet been investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay-Lussac and Thenard is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oximuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid; and the liquid, I believe, only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oximuriatic acid.

Their decomposition by water a clew to the porportions of elements in oxides, acids, and alkaline earths.

It is evident, that, whenever an oximuriatic combination is decomposed by water, the oxide, or acid, or alkali, or oxidated body formed, must be in the same proportion as the muriatic acid gas, as the oxigen and hidrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements in the different oxides, acids, and alkaline earths.

Weights of different elements as data.

If, according to the ingenious idea of Mr. Dalton, hidrogen be considered as 1 in weight, in the proportion it exists in water, then oxigen will be nearly 7·5; and assuming that potash is composed of 1 proportion of oxigen, and 1 of potassium, then potash will be 48, and potassium* about 40·5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oximuriatic acid will be represented by 32·9, and muriatic acid gas, of course, by 33·9; and this estimation agrees with the specific gravity of oximuriatic acid gas, and muriatic acid gas. From my experiments, 100 cubical inches of oximuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains; whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxigen with which the inflammable matter will combine †.

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* Supposing potash to contain nearly 15·6 per cent of oxigen.

† I have stated in the last Bakerian lecture, that, during the decomposition of the amalgam from ammonia, 1 in volume of hidrogen to 2 of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. *If* it be supposed that the hidrogen arises from the decomposition of water; then the oxigen, that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hidrogen in an equal volume of muriatic acid; or if it be said, that ammonium is a compound of 2 of ammonia and 1 of hidrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oximuriatic acid and ammonium, supposing they could be immediately combined. I once thought, that the phenomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, the metal of ammonia, in which hidrogen was so loosely combined as to be separable with great ease,

Modified phlogistic theory.

Potassium does not form hydrat of potash by combustion.

In considering the dry muriates as compounds of oximuriatic acid and inflammable bodies, the argument that I have used in the last Bakerian lecture, to show, that potassium does not form hydrat of potash by combustion, is considerably strengthened; for from the quantity of oximuriatic acid the metal requires to produce a muriate, it seems to be shown, that it is the simplest known form of the alkaline matter. This I think approaches to an *experimentum crucis*. Potash made by alcohol, and that has been heated to redness, appears to be a hydrat of potash; while the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent of water to convert it into a hydrat.

Charcoal does not combine directly with oximuriatic acid, but forms triple compounds with it and hidrogen.

Among all the known combustible bodies charcoal is the only one, which does not combine directly with oximuriatic acid gas; and yet there is reason for believing, that this combination may be formed by the intermedium of hidrogen. I am inclined to consider the oily substance, produced by the action of oximuriatic acid gas and olefiant gas, as a ternary compound of these bodies; for they combine nearly in equal volumes: and I find, that by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor, and muriatic ether, as is probable from the ingenious experiments of Mr. Gehlen and Mr. Thenard, must be combinations of a similar kind, one probably with more hidrogen, and the other with more carbon.

ease, and in which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxigen. The second, the metals of the alkalis and alkaline earths, in which the hidrogen was more firmly combined, but in combustion forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hidrogen was more intimately combined; producing by union with oxigen, water not separable by any new attractions. The phenomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations, so far as they concern the metals from the fixed alkalis.

very question-able.

Decomposition of the muriates of potash and soda,

One of the greatest problems in economical chemistry is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oximuriatic acid is very strong: but so likewise is their attraction for oxigen, and the affinity of their oxides for water. The affinities of oximuriatic acid gas for hidrogen, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases, when it is intended to attempt to produce alkali. It is not difficult after these views to explain the decomposition of common salt by aluminous or silicious substances, which, as it has been long known, act only when they contain water. In these cases the sodium may be conceived to combine with the oxigen of the water and with the earth, to form a vitreous compound; and the oximuriatic acid to unite with the hidrogen of the water, forming muriatic acid gas.

and of salt by moistened litharge.

It is also easy, according to these new ideas, to explain the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity: the oximuriatic acid is attracted by the lead, and the sodium combines with the oxigen of the litharge and with water to form hydrat of soda, which gradually attracts carbonic acid from the air.

Salt decomposed by passing steam over a heated mixture of it with iron filings.

As iron has a strong affinity for oximuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings and muriate of soda intensely heated: and in this way I succeeded in decomposing some of the salt: hidrogen came over; a little hidrate of soda was formed; and muriate of iron was produced.

Potassium and sodium may be procured from their oximuriatic compounds.

It does not seem improbable, supposing the views that have been developed accurate, that, by complex affinities, even potassium and sodium in their metallic form may be procured from their oximuriatic combinations. For this purpose the oximuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society to some of the theoretical relations of the facts noticed in the preceding pages.

Extraordinary nature of the compound of oximuriatic acid and ammonia.

That a body principally composed of oximuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phenomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form, in this instance, a substance neither fusible nor volatile at a white heat. It could not have been expected, that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oximuriatic acid would have appeared incredible, according to all the existing analogies of chemistry. The experiments, on which these conclusions are founded, are, however, uniform in their results; and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oximuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common reagents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? And that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Complexity of composition not always connected with facility of decomposition.

Other compounds of the oximuriatic acid.

Beside the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable, that other compounds of like nature may be formed of the oxides, alkalis, and earths, with the oximuriatic combinations, or of the oximuriatic compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and, as it would seem at first view, contradictory facts. For if, as I have said, oximuriatic acid gas be referred to the same class of bodies as oxigen gas, then, as oxigen is not an acid, but forms acids by combining with certain inflammable bodies, so oximuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hidrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oximuriates of phosphorus and tin.

which appears to be analogous with oxigen.

Like oxigen, oximuriatic acid is attracted by the positive surface in voltaic combinations; and on the hypothesis of the connection of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

(To be concluded in our next.)

II.

Observations upon Luminous Animals. By J. MACARTNEY, *Esq. Communicated by* EVERARD HOME, *Esq. F. R. S.*[*]

THE property, which certain animals possess of emitting light, is so curious and interesting, that it has attracted the attention of naturalists in all ages. It was particularly noticed by Aristotle and Pliny among the ancients; and the publications of the different learned societies in Europe

Luminous animals have attracted much attention,

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[*] Philos. Trans. for 1810, p. 258.

But our knowledge of them very imperfect.

contain numerous memoirs upon the subject. Notwithstanding the degree of regard bestowed upon the history of luminous animals, it is still very imperfect; the power of producing light appears to have been attributed to several creatures which do not possess it; some species, which enjoy it in an eminent degree, have been imperfectly described, or entirely unobserved: the organs which afford the light in certain animals have not been examined by dissection; and lastly, the explanations that have been given of the phenomena of animal light are unsatisfactory, and in some instances palpably erroneous.

The author has long studied them,

and received valuable communications from Sir J. Banks.

As this subject forms an interesting part of the history of organized beings, I have for some years availed myself of such opportunities as occurred for its investigation. Having communicated the result of some of my researches to the Right Honourable Sir Joseph Banks, he immediately offered me his assistance with that liberality, which so eminently distinguishes him as a real lover of science. I am indebted to him for an inspection of the valuable journal he kept during his voyage with Captain Cook; for permission to copy the original drawings, in his possession, of those luminous animals discovered in both the voyages of Cook; and for some notes upon the luminous appearance of the sea, that were presented to him by Captain Horsburg, whose accuracy of observation is already known to this learned Society.

Plan of the paper.

In the following paper, I shall first examine the grounds on which the property of showing light has been ascribed to certain animals, that either do not possess it, or in which its existence is questionable. I shall next give an account of some luminous species, of which some have been inaccurately described, and others quite unknown. I shall endeavour to explain from my own observations, and the information communicated to me by others, many of the circumstances attending the luminous appearance of the sea. I shall then describe the organs employed for the production of light in certain species; and lastly, I shall review the opinions which have been entertained respecting the nature and original of animal light, and relate the experiments I have made for the purpose of elucidating this part of the subject.

The

Luminousness ascribed erroneously to certain fishes;

The property of emitting light has been reported to belong to several fishes, more particularly the mackarel, the moonfish (tetraodon mola), the dorado, mullet, sprat, &c.

Mr. Bajon observed during the migration of the dorados, &c., that their bodies were covered with luminous points. These however proved upon examination to be minute spherical particles, that adhered to the surface of these fishes; and, he adds, appeared to be precisely the same sort of points, that illuminated the whole of the sea at the time. They were therefore in all probability the minute kind of medusa, which I shall have occasion to describe hereafter.

Godeheu de Riville states, in a paper sent to the Academy of Sciences at Paris, that, on opening the scomber pelamis while alive, he found in different parts of its body an oil which gave out much light: but it should be observed, that Riville had a particular theory to support, for which this fact was very convenient; and that other parts of his memoir bear marks of inaccuracy. It may be added, that, if the oil of fishes were usually luminous, which Riville supposed, it would be almost universally known, instead of resting on a solitary observation.

(but no fishes exhibit light while living;)

As far as I am able to determine from what I have seen, the faculty of exhibiting light during life does not belong to the class of fishes. It appears probable, that some fishes may have acquired the character of being luminous, from evolving light soon after death.

to some vermes;

Some species of lepas, murex, and chama, and some starfish have been said to possess the power of shining; and the assertion has been repeated by one writer after another, but without quoting any authority.

earthworms;

Brugueire upon one occasion saw, as he supposed, common earthworms in a luminous state; all the hedges were filled with them; he remarked, that the light resided principally in the posterior part of the body*.

Flaugergues pretended to have seen earthworms luminous in three instances; it was at each time in October; the body shone at every part, but most brilliantly at the genital organs*.

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* Journal d'Histoire Naturelle, Tom. II.

Notwithstanding this concurrence of testimony, it is next to impossible, that animals, so frequently before our eyes as the common earthworm, should be endowed with so remarkable a property, without every person having observed it. If they only enjoyed it during the season for copulation, still it could not have escaped notice, as these creatures are usually found joined together in the most frequented paths, and in garden walks.

the water flea;

In different systems of natural history, the property of shining is attributed to the cancer pulex. The authorities for this opinion are Hablitzl, and Thules and Bernard. The former observed upon one occasion a cable that was drawn up from the sea exhibit light, which upon closer inspection was perceived to be covered by these insects †. Thules and Bernard reported, that they met with a number of this species of cancer on the borders of a river entirely luminous ‡. I am nevertheless disposed to question the luminous property of the cancer pulex, as I have often had the animal in my possession, and never perceived it emit any light.

and the scolopendra phosphorea.

The account given by Linneus of the scolopendra phosphorea is so improbable and inconsistent, that one might be led to doubt this insect's existence, particularly as it does not appear to have been ever seen, except by Ekeberg, the captain of an East Indiaman, from whom Linneus learnt its history.

I now proceed to the description of those luminous animals, that have been discovered by the Right Honourable Sir Joseph Banks, Captain Horsburg, and myself.

Two luminous marine animals discovered by Sir J. Banks.

On the passage from Madeira to Rio de Janeiro, the sea was observed by Sir Joseph Banks to be unusually luminous, flashing in many parts like lightning. He directed some of the water to be hauled up, in which he discovered two kinds of animals, that occasioned the phenomenon; the one, a crustaceous

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* Journal de Physique, Tome XVI.

† Hablitzl ap. Pall. n. Nord. Beytr. 4, p. 396.

‡ Journal de Physique, Tome XXVIII.

Fig 1

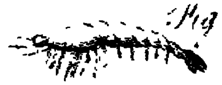


Fig 2.

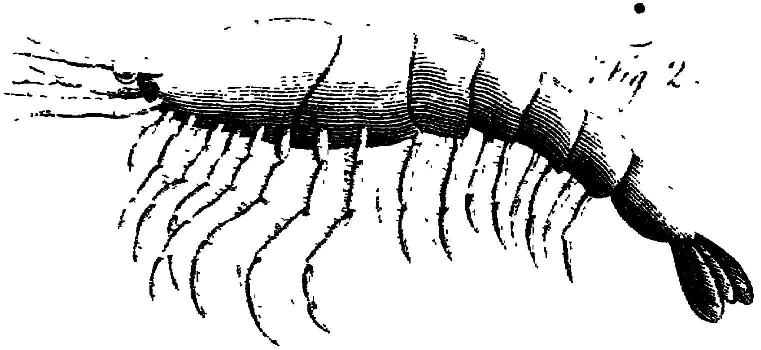
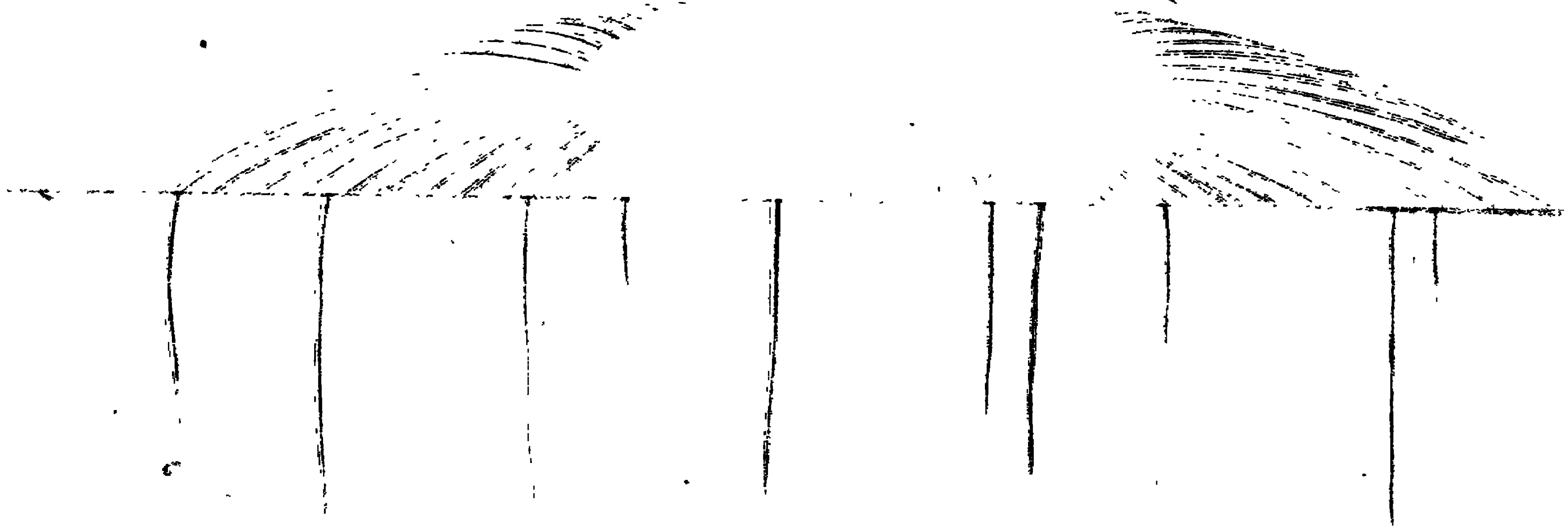


Fig 3.



crustaceous insect, which he called the cancer fulgens; the other, a large species of medusa, to which he gave the name of pellucens.

Cancer fulgens.

The cancer fulgens bears some resemblance to the common shrimp; it is however considerably less. The legs are furnished with numerous setæ. The light of this animal, which is very brilliant, appears to issue from every part of the body. See it Pl. IX, fig. 1, of the natural size, and magnified at fig. 2.

Medusa pellucens.

The medusa pellucens measures about six inches across the crown or umbella; this part is marked by a number of opake lines, that pass off from the centre to the circumference. The edge of the umbella is divided into lobules, which succeed each other, one large and two small ones alternately. From within the margin of the umbella there are suspended a number of long cord-shaped tentacula. The central part of the animal is opake, and furnished with four thick irregularly shaped processes, which hang down in the midst of the tentacula. See fig. 3.

This zoophyte is the most splendid of the luminous inhabitants of the ocean. The flashes of light emitted during its contractions are so vivid, as to affect the sight of the spectator.

Shining of the sea observed by captain Horsburg.

In the notes communicated to Sir Joseph Banks by Captain Horsburg, he remarks, that the luminous state of the sea between the tropics is generally accompanied with the appearance of a great number of marine animals of various kinds upon the surface of the water: to many of which he does not, however, attribute the property of shining. At other times, when the water which gave out light was examined, it appeared only to contain small particles of a dusky straw colour, which dissolved with the slightest touch of the finger. He likewise observes, that in Bombay during the hot weather of May and June, he has frequently seen the edges of the sea much illuminated by minute sparkling points.

Luminous insect found by him.

At sunrise, on April 12, 1798, in the Arabian sea, he perceived several luminous spots in the water, which conceiving to be animals, he went in the boat and caught one. It proved to be an insect somewhat resembling in appearance the woodlouse, and was about one third of an inch in length. When viewed with the microscope, it seemed to be formed by sections of a thin crustaceous substance. During the time that any fluid remained in the animal, it shone brilliantly like the fire fly.

Another.

In the month of June in the same year, he picked up another luminous insect on a sandy beach, which was also covered with a thin shell, but it was of a different shape, and a larger size than the animal taken in the Arabian sea.

Both monoculi.

By comparing the above description with an elegant pen and ink drawing, which was made by Captain Horsburg, and accompanied his paper, I have no doubt, that both these insects were monoculi; the first evidently belongs to the genus limulus of Muller; I shall therefore beg leave to distinguish it by the name of limulus noctilucus.

Luminous vermes discovered by the author.

My pursuits, and the state of my health, having frequently led me to the coast, I have had many opportunities of making observations upon the animals, which illuminate our own seas. Of these I have discovered three species: one of which is a beroe not hitherto described by authors; another agrees so nearly with the medusa hemispherica, that I conceive it to be the same, or at least a variety of that species; the third is a minute species of medusa, which I believe to be the luminous animal, so frequently seen by navigators, although it has never been distinctly examined or described.

Minute luminous medusæ described.

I first met with these animals in the month of October 1804, at Herne Bay, a small watering place upon the northern coast of Kent. Having observed the sea to be extremely luminous for several nights, I had a considerable quantity of the water taken up. When perfectly at rest, no light was emitted, but on the slightest agitation of the vessel in which the water was contained, a brilliant scintillation was perceived, particularly towards the surface; and when the vessel was suddenly struck, a flash of light issued from the top of the water, in consequence of so many points shining at the same moment. When any of these sparkling points were removed from the water, they no longer yielded any light. They were so transparent, that in the air they appeared like globules of water. They were more minute than the head of the smallest pin. Upon the slightest touch, they broke and vanished from the sight. Having strained a quantity of the luminous water, a great number of these transparent corpuscles were obtained upon the cloth; and the water, which had been strained, did not afterward exhibit the least light. I then put some sea water, that had been rendered particularly clear by repeated filtrations, into a large glass; and having floated in it a fine cloth, on which I had previously collected a number of luminous points, several of them were liberated, and became distinctly visible in their natural element, by placing the glass before a piece of dark coloured paper. They were observed to have a tendency to come to the surface of the water, and after the glass was set by for some time, they were found congregated together, and when thus collected in a body, they had a dusky straw colour, although individually they were so transparent, as to be perfectly invisible, except under particular circumstances. Their substance was indeed so extremely tender and delicate, that they did not become opaque in distilled vinegar or alcohol, until immersed in these liquors for a considerable time.

On examining these minute globules with the microscope, I found that they were not quite perfect spheres, but had an irregular depression on one side, which was formed of an opaque substance, that projected a little way inwards, producing such an appearance as would arise from tying the neck of a round bag, and turning it into the body.

The motions of these creatures in the water were slow and graceful, and not accompanied by any visible contraction of their bodies. After death they always subsided to the bottom of the vessel.

Medusa scintillans.

From the sparkling light afforded by this species, I shall distinguish it by the name of medusa scintillans.

Beroe fulgens described.

The night following that, on which I discovered the preceding animal, I caught the two other luminous species. One of these I shall call the beroe fulgens.

This most elegant creature is of a colour changing between purple, violet, and pale blue; the body is truncated before, and pointed behind; but the form is difficult to assign, as it is varied by partial contractions, at the animal's pleasure. I have represented the two extremes of form, that I have seen this creature assume: the first is somewhat that of a cucumber, which, as being the one it takes when at rest, should perhaps be considered as its proper shape: the other resembles a pear, and is the figure it has in the most contracted state. The body is hollow, or forms internally an infundibular cavity, which has a wide opening before, and appears also to have a small aperture posteriorly, through which it discharges its excrement. The posterior two thirds of the body are ornamented with eight longitudinal ciliated ribs, the processes of which are kept in such a rapid rotatory motion, while the animal is swimming, that they appear like the continual passage of a fluid along the ribs. The ciliated ribs have been described by Professor Mitchell as arteries, in a luminous beroe, which I suspect was no other than the species I am now giving an account of.

Light emitted by it.

When the beroe fulgens swam gently near the surface of the water, its whole body became occasionally illuminated in a slight degree; during its contractions, a stronger light issued from the ribs, and when a sudden shock was communicated to the water, in which several of these animals were placed, a vivid flash was thrown out. If the body were broken, the fragments continued luminous for some seconds, and being rubbed on the hand, left a light like that of phosphorus; this however, as well as every other mode of emitting light, ceased after the death of the animal.

Hemispherical medusa.

The hemispherical species of medusa, that I discovered, had a very faint purple colour. The largest that I found, measured about three quarters of an inch in diameter. The margin of the umbella was undivided, and surrounded internally by a row of pale brown spots, and numerous small twisted tentacula: four opaque lines crossed in an arched manner from the circumference towards the centre of the animal: an opaque irregular shaped process hung down from the middle of the umbella: when this part was examined with a lens of high powers, I discovered that it was inclosed in a sheath in which it moved, and that the extremity of the process was divided into four tentacula, covered with little cups or suckers, like those on the tentacula of the cuttlefish.

Resembles those of Gronovius and Muller.

This species of medusa bears a striking resemblance to the figures of the medusa hemispherica, published by Gronovius and Muller; indeed it differs as little from these figures, as they do from each other. Its luminous property, however, was not observed by these naturalists; which is the more extraordinary, as Muller examined it at night, and says it is so transparent, that it can only be seen with the light of a lamp. If it should be still considered as a distinct species, or as a variety of the hemisphcrica, I would propose to call it the medusa lucida.

Mode of its shining.

In this species, the central part and the spot round the margin are commonly seen to shine on lifting the animal out of the water into the air, presenting the appearance of an illuminated wheel; and when it is exposed to the usual percussion of the water, the transparent parts of its body are alone luminous.

These animals retreated from the surface of the sea when the moon rose, and did not shine in daylight.

In the month of September 1805, I again visited Herne Bay, and frequently had opportunities of witnessing the luminous appearance of the sea. I caught many of the hemispherical and minute species of medusa, but not one of the beroe fulgens. I observed, that these luminous animals always retreated from the surface of the water, as soon as the moon rose. I found also, that exposure to the day light took away their property of shining, which was revived by placing them for some time in a dark situation.

Large flashes of light on the sea from them.

In that season I had two opportunities of seeing an extended illumination of the sea, produced by the above animals. The first night I saw this singular phenomenon was extremely dark, many of the medusa scintillans, and medusa hemispherica had been observed at low water, but on the return of the tide, they had suddenly disappeared. On looking towards the sea, I was astonished to perceive a flash of light of about six yards broad, extend from the shore, for apparently the distance of a mile and a half along the surface of the water. The second time that I saw this sort of light proceed from the sea, it did not take the same form, but was diffused over the surface of the waves next the shore, and was so strong, that I could for the moment distinctly see my servant, who stood at a little distance from me; he also perceived it, and called out to me at the same instant. On both these occasions the flash was visible for about four or five seconds, and although I watched for it a considerable time, I did not see it repeated.

Diffused luminous appearance of the sea.

A diffused luminous appearance of the sea, in some respects different from what I have seen, has been described by several navigators.

Godeheu de Riville saw the sea assume the appearance of a plain of snow on the coast of Malabar*.

Other similar appearances at particular times.

Captain Horsburg, in the notes he gave to Sir Joseph Banks, says, there is a peculiar phenomenon sometimes seen within a few degrees distance of the coast of Malabar, during the rainy monsoon, which he had an opportunity of observing. At midnight the weather was cloudy, and the sea was particularly dark, when suddenly it changed to a white flaming colour all around. This bore no resemblance to the sparkling or glowing appearance he had observed on other occasions in seas near the equator, but was a regular white colour, like milk, and did not continue more than ten minutes. A similar phenomenon, he says, is frequently seen in the Banda sea, and is very alarming to those, who have never perceived or heard of such an appearance before.

Striking instance of this phenomenon,

This singular phenomenon appears to be explained by some observations communicated to me by Mr. Langstaff, a surgeon in the city, who formerly made several voyages. In going from New Holland to China, about half an hour after sunset, every person on board was astonished by a milky appearance of the sea: the ship seemed to be surrounded by ice covered with snow. Some of the company supposed they were in soundings, and that a coral bottom gave this curious reflection, but on sounding with 70 fathoms of line no bottom was met with. A bucket of water being hauled up, Mr. Langstaff examined it in the dark, and discovered a great number of globular bodies, each about the size of a pin's head, linked together. The chains thus formed did not exceed three inches in length, and emitted a pale phosphoric light. By introducing his hand into the water, Mr. Langstaff raised upon it several chains of the luminous globules; which were separated by opening the fingers, but readily reunited on being brought again into contact, like globules of quicksilver. The globules, he says, were so transparent, that they could not be perceived when the hand was taken into the light.

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* Mém. Etrang. de l'Acad. des Sc. Tom. 3.

This extraordinary appearance of the sea was visible for two nights. As soon as the moon exerted her influence, the sea changed to its natural dark colour, and exhibited distinct glittering points, as at other times. The phenomenon, he says, had never been witnessed before by any of the company on board, although some of the crew had been two or three times round the globe.

which continued for two nights, but was dissipated by the moonlight.

I consider this account of Mr. Langstaff very interesting and important, as it proves, that the diffused light of the sea is produced by an assemblage of minute medusæ on the surface of the water.

In June 1806, I found the sea at Margate more richly stored with the small luminous medusæ, than I have ever seen it. A bucket of the water being set by for some time, the animals sought the surface, and kept up a continual sparkling, which must have been occasioned by the motions of individuals, as the water was perfectly at rest. A small quantity of the luminous water was put into a glass jar, and on standing some time, the medusæ collected at the top of the jar, and formed a gelatinous mass, one inch and a half thick, and of a reddish or mud colour, leaving the water underneath perfectly clear.

Number of the small luminous medusæ caught at Margate.

In order to ascertain if these animals would materially alter their size, or assume the figure of any other known species of medusa, I kept them alive for 25 days, by carefully changing the water in which they were placed; during which time, although they appeared as vigorous as when first taken, their form was not in the slightest degree altered, and their size but little increased. By this experiment I was confirmed in the opinion of their being a distinct species, as the young actiniæ and medusæ exhibit the form of the parent in a much shorter period than the above.

They did not alter their form, and scarcely their size in 25 days.

Beroe fulgens of various sizes.

In September, 1806, I took at Sandgate a number of the beroe fulgens, but no other species: they were of various dimensions, from the full size down to that of the medusa scintillans: they could however be clearly distinguished from the latter species, by their figure.

Medusa scintillans on various parts of our coasts.

Since that time, I have frequently met with the medusa scintillans on different parts of the coast of Sussex, at Tenby, and at Milford haven. I have likewise seen this species in the bays of Dublin and Carlingford in Ireland.

Number of the beroe fulgens caught.

Cause of the diffused light on the ocean,

and of the flashes in Herne bay.

In the month of April, last year, I caught a number of the beroe fulgens in the sea at Hastings: they were of various sizes, from about the half of an inch in length, to the bulk of the head of a large pin. I found many of them adhering together in the sea; some of the larger sort were covered with small ones, which fell off when the animals were handled; and, by a person unaccustomed to observe these creatures, would have been taken for a phosphoric substance. On putting a number of them into a glass, containing clear sea water, they still showed a disposition to congregate upon the surface. I observed, that, when they adhered together, they showed no contractile motion in any part of their body, which explains the cause of the pale or white colour of the diffused light of the ocean. The flashes of light, which I saw come from the sea at Herne bay, were probably produced by a sudden and general effort of the medusæ to separate from each other, and descend in the water.

Medusa scintillans abounds in Milford haven.

Probably affect the density of the sea.

The medusa scintillans almost constantly exists in the different branches of Milford haven, that are called pills. I have sometimes found these animals collected in such vast numbers in those situations, that they bore a considerable proportion to the volume of the water in which they were contained: thus, from a gallon of sea water in a luminous state I have strained above a pint of these medusæ. I have found the sea under such circumstances to yield me more support in swimming, and the water to taste more disagreeably than usual; probably the difference of density, that has been remarked at different times in the water of the sea, may be referred to this cause.

The most frequent source of the luminous-

All my own observations lead me to conclude, that the medusa scintillans is the most frequent source of the light ness of the sea. Mistaken for the nereis noctiluca by some; and described by others, who were not aware of its nature.

of the sea around this country; and by comparing the accounts of others with each other, and with what I have myself seen, I am persuaded, that it is so likewise in other parts of the world. Many observers appear to have mistaken this species for the nereis noctiluca, which was very natural, as they were prepossessed with the idea of the frequent existence of the one, and had no knowledge of the other. Some navigators have actually described this species of medusa, without being aware of its nature. Mr. Bajon, during his voyage from France to Cayenne, collected many luminous points in the sea, which he says, when examined by a lens, were found to be minute spheres. They disappeared in the air. Doctor Le Roy, in sailing from Naples to France, observed the sparkling appearance of the sea, which is usually produced by the medusa scintillans. By filtering the water, he separated luminous particles from it, which he preserved in spirit of wine: they were, he says, like the head of a pin, and did not at all resemble the nereis noctiluca, described by Vianelli; their colour approached a yellow brown, and their substance was extremely tender, and fragile. Notwithstanding this striking resemblance to the medusa scintillans, Le Roy, in consequence of a preconceived theory, did not suppose what he saw were animals, but particles of an oily or bituminous nature *.

The minute globules, seen by Mr. Langstaff in the Indian ocean, were, I think, in all probability, the scintillating species of medusa: and on my showing him some of these animals I have preserved in spirits, he entertained the same opinion.

Professor Mitchell, of New York, found the luminous appearance on the coast of America, to be occasioned by minute animals, this, from his description, plainly belonged to this species of medusa, notwithstanding which, he supposed them to be a number of the nereis noctiluca †.

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* Observ. sur une Lumière produite par L'Eau de la Mer. *Mém.* Etrang. des Sc.

† Phil. Mag. Vol. X, p. 20.

The luminous animalcule of Forster.

The luminous animalcule, discovered by Forster off the Cape of Good Hope, in his voyage round the world, bears so stronga resemblance to the medusa scintillans, that I am much disposed to believe them the same. He describes his animalcule as being a little gelatinous globule, less than the head of a pin; transparent, but a little brownish in its colour; and of so soft a texture, that it was destroyed by the slightest touch. On being highly magnified, he perceived on one side a depression, in which there was a tube that passed into the body, and communicated with four or five intestinal sacs. The pencil drawings he made on the spot are in the possession of Sir Joseph Banks, by whose permission engravings from them are subjoined to this paper. By comparing these with the representations of the medusa scintillans, and some of this species rendered visible, by being a long time preserved in spirits, which I have laid before this learned society, it will be found, that the only difference between Forster's animalcule, and the medusa scintillans, is in the appearance of the opaque parts, shown in the microscopic views.

Luminousness of the sea erroneously ascribed to various causes.

Many writers have ascribed the light of the sea to other causes than luminous animals. Martin supposed it to be occasioned by putrefaction: Silberschlag believed it to be phosphoric: professor J. Mayer conjectured, that the surface of the sea imbibed light, which it afterward discharged. Bajon and Gentil thought the light of the sea was electric, because it was excited by friction. Forster conceived, that it was sometimes electric, sometimes caused from putrefaction, and at others by the presence of living animals. Fougeroux de Bondaroy believed, that it came sometimes from electric fires, but more frequently from the putrefaction of marine animals and plants.

I shall not trespass on the time of the Society, to refute the above speculations; their authors have left them unsupported by either arguments or experiments, and they are inconsistent with all ascertained facts upon this subject.

(To be concluded in our next.)

III. *Note*

III.

Note on the Water contained in fused Soda. By Mr. J. E. Berard[*].

Mr. d'Arcet had found, that pure alkalis, after being fused, contained water, and he estimated the quantity in soda as high as 28 per cent [†]. An analysis, which I made some time after in a different mode, gave me but 18·86.

Water in soda after fusion.

The result of Mr. d'Arcet is founded on the analysis of the subcarbonate of soda. For this salt too he has given proportions different from those I found [‡]. Not knowing the particulars of Mr. d'Arcet's experiments, I ascribed the difference of our results respecting the water in soda to the difference in these proportions. In confirmation of his opinion however, he has just published these experiments; and has made several objections to my method of analysing the subcarbonate, which I shall endeavour to remove.

Mr. d'Arcet gives the proportions of subcarbonate of soda different from the author,

and makes to his method

He considers the solution in acids as an inaccurate mode; 1st, because the solution retains carbonic acid: 2dly, because the gas evolved carries off water with it.

two objections.

It may be observed, that, these sources of errour being opposite in their effects, they in great measure counteract each other: but we shall see what is their extent. The temperature amounts at most to 20° [68° F], when the carbonate decomposed by the acid is already in solution. Now at this temperature water under the pressure of the atmosphere alone would not dissolve any thing like its own bulk of carbonic acid gas. But this quantity may be wholly neglected, when we consider, that a large excess of sulphuric acid is added, and the solution strongly shaken.

These errours tend to balance each other

The first must be of very small amount.

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[*] Annales de Chim. vol. lxxii, p. 96.

[†] Ann. de Chim. vol. lxviii, p. 175; or Journal, p. 31 of the present vol.

[‡] For the component parts of different salts by Mr. Berard see Journal, vol. xxvi, p. 206.

As

and the same is true of the second.

As to the water carried off by the gas, when the experiment is performed with due caution in proper vessels, the gas only carries with it hygrometrical water. Now this quantity may easily be calculated. The gas in my experiment weighed 4·195 gram. [64·77 grs.]: consequently its volume, at 20° [68°F.] and 0·75 met. [29·5 in.] pressure, was 2·3 lit. [4·8 pints]; and the weight of the aqueous vapour =4·2 cent. [0·65 of a gr.]. If this quantity be diminished by the very small quantity of carbonic acid retained in the solution*, we shall have the extent of the errour, to which this analysis is liable; for the process is so simple, that on repeating it we obtain almost precisely the same quantities. I conceive it much to be wished, that all analyses were susceptible of this precision: besides, I have confirmed it by other experiments. I reduced the subcarbonate to muriate and sulphate, and the results I obtained were found to agree with the analysis I had adopted.

Objections to the mode of determining the water in soda.

I now proceed to the objections which Mr. d'Arcet has made to my method of determining the water in soda, and which consist in this, that the component parts of muriate of silver are not accurately determined; and that this salt is soluble in the waters of elutriation.

Proportions of muriate of silver ascertained.

Chenevix, Zaboade, Proust, Bucholz, Rose, and others, have successively determined the proportions of muriate of silver. The quantity of acid contained in this salt varies in their results from 17 to 18. Rose and Bucholz make it 17·5, which is the proportion I have adopted. The late experiments of Gay-Lussac carry it to 18. When the proportions assigned to a salt vary only half a hundredth part, they may be fairly considered as ascertained.

This muriate is among the most insoluble salts.

With regard to the solubility of muriate of silver, I believe we may affirm, that it is one of the most insoluble salts employed in analysis, when the liquid is neutral and contains very little of any other salt. It even requires a pretty considerable excess of acid, to dissolve any notable quantity. Nitrate of silver is such a powerful test of muriatic acid, that it detects extremely small quantities. Kirwan found, that one part of muriatic acid diluted with 108333 of water could be detected by nitrate of silver*. Does muriate of lime indicate such imperceptible quantities of carbonic acid, particularly in a liquid that is not neutral?

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* There is still another circumstance, that tends to diminish the weight of the carbonic acid: this is, the gas evolved expels great part of the atmospheric air contained in the empty part, and takes its place.

Causes of difference in the results.

These observations seem to me to answer entirely the objections Mr. d'Arcet has made to my analysis: though at the same time I am far from considering his method as a bad one. Whence then, perhaps, it will be asked, arises the difference between the results? I conceive it originates from the proportions, which Mr. d'Arcet has adopted for the carbonate of lime, and from the difficulty he must have found to deprive this salt entirely of water, without expelling some of its acid.

Component parts of carbonate of lime.

Thenard and Biot have just made a comparative analysis of carbonate of lime and arragonite. In their experiments they employed all the care, that might be expected from such experienced chemists. From these it appeared, that 56 of lime unite with 43 of carbonic acid.

Calculation from these

But Mr. d'Arcet has found, that 100 parts of crystallized subcarbonate of soda, containing 36·39 of dry subcarbonate, gave 34·81 of carbonate of lime, which, exposed to a strong fire, left 18·72 of quicklime. This quantity of lime, from the analysis above quoted, would combine with 14·37 of carbonic acid, which were accordingly contained in the 36·39 of dry subcarbonate: and my analysis would have given 13·63. If it be considered too, that I was obliged to take that analysis of Mr. d'Arcet's, in which he found most carbonate of lime, these results, I imagine, will not be thought very wide.

brings the results very near.

Now if Mr. d'Arcet's experiments be calculated from my analysis of the subcarbonate of soda, it will appear, that 100 parts of the soda he analysed contain 20 of water, while I found 18·86.

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[*] I am aware, that Mr. Berthollet junr. has detected much smaller quantities of acid with the same test. It is true, he found, that muriate of silver was soluble in concentrated and boiling solutions of almost all muriates: but it is sufficient to dilute them with water, to occasion its reappearance.

IV.

On a new Pitchlike Iron Ore, or Sulphated Iron with Excess of Base: by Mr. Gillet Laumont, *Correspondent of the Institute, and Member of the Council of Mines*[*].

New and rare mineral from Ferber's collection,

called by him pitchlike iron ore.

Mr. Karsten has just sent to Mr. Haüy a new and very rare species of mineral. Examining the geographical collection of the department of Mines at Berlin, that gentleman found among the Saxon minerals from Ferber's collection, which had been purchased by that department, a small box, labelled by Ferber "Pitchlike iron ore from the mine of Kustbescheerung near Freyberg."

Other minerals similarly named.

In his letter to Mr. Haüy Mr. Karsten observes, that mineralogists have confounded the pitchlike iron ore sometimes with the black blende of Freyburg, at other times with the oxidule of uranium, and that latterly Werner has given this name to Haüy's phosphated manganese[†].

Its component parts.

Mr. Karsten quickly perceived, that this mineral did not agree with any of those known by this denomination; and his conjecture was confirmed by Klaproth, who on analysing it obtained

Oxide of iron - - - -	67
Dry sulphuric acid - - -	8
Water - - - - -	25
	100

Its characters.

The specific gravity of this mineral was 2·144. The specimens sent to Mr. Haüy are small and very brittle, but varying in colour. A piece being heated in the flame of a candle swelled up, fused pretty readily, and became magnetic.

Similar substance from a mine in Brittany.

I gave a description of a substance much resembling this in a paper on the mines of Brittany, which I presented to the Academy of Sciences in May, 1786. It was there called "an acid phosphoric martial salt." I brought it from the lead mine of Huelgoat, celebrated for the phosphates of lead, which I then made known[‡]. It was among these lead ores, at the depth of 494 feet [527 Eng.], that this resiniform substance was found. It appeared to have been melted over several pieces, which were impregnated with it; and was commonly in very brittle, drusy masses.

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[*] Journal des Mines, vol. xxiii, p. 221.

[†] Jameson's Mineralogy, vol. ii, p. 569, 612. C.

[‡] I obtained at that time from these lead ores four or five per cent

The two compared.

I have only a few fragments of this left, and they exhibit nearly the same characters as Ferber's pitchlike iron ore, which may now be termed sulphated iron with excess of base. Dr. Weiss, of Leipsic, who was present at some experiments I lately made with the two substances, did not hesitate to consider them as the same. In fact, the substance of Huelgoat and that of Freyberg have both a deep yellow colour, varying between that of olivine and idiocrase: their aspect is equally resinous; their fracture is conchoidal, unequal, shining; their hardness superior to that of sulphate of lime, but inferior to that of carbonate of lime, which scratches them strongly, though they also scratch it slightly; their brittleness is very great; their colour, when scraped, is yellow, though that of the Freyberg mineral is a little deeper than the other.

Their characters.

In the flame of a candle, before the blowpipe, they swell up, and crack, assuming the colour of gamboge in the lump; and at length melt into black scoriæ, attractable by the magnet, which adhere to the platina tongs. The mineral from Freyberg only swells up more, melts more quickly, and adheres more to the tongs.

Evidence of sulphuric acid.

When fresh, the resiniform mineral of Huelgoat gave a white precipitate with the solution of barytes in muriatic acid, which indicated the presence of sulphuric acid; and it impressed then an acid and styptic taste on the tongue, which it has now lost: but the presence of sulphuric acid in it has been confirmed afresh by Mr. Descotils, though he had not enough to verify that of phosphoric acid.

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cent of phosphorus, by a simple, new, and speedy method, described in that paper. I was led to it by the green flame, which I observed to rise from the crucibles in which I heated the ore. In the same way I was induced to suspect the presence of phosphorus in the resiniform ore accompanying this lead from the green flame I obtained with the blowpipe, and the white precipitate it gave with lime-water. See Journ. de Phys., May, 1786, vol. xxviii, p. 352, 385.

V.

Analysis of three Species of Pyrites, by Mr. Bucholz *.

Analysis of pyrites.

MESSRS. Gehlen and Bucholz having particular reasons for wishing to examine the observations of Proust on the two compounds of iron and sulphur, they made their experiments on an artificial sulphuret with a maximum of sulphur, because they had not any native. Having afterward procured three different specimens of native pyrites; Mr. B. determined to analyse them carefully, in consequence the difference between the results of some eminent chemists. The following are the component parts of pyrites:

Component parts according to different authors.

	Sulphur.	Iron.
according to		
Proust - - - - -	47·36	52·64
Hatchet - - - - -	53·24	46·76
Gueniveau † - - -	52·76	47·24
	53·40	46·60
	53·69	46·31
Bucholz and Gehlen -	47·93	52·07
	47·36	52·64

Usual process consumes much time and acid.

As the process generally adopted, which consists in treating pyrites with weak nitric acid, requires a great deal of acid and of time, Mr. Bucholz first endeavoured to find a process, that should convert the pyrites into oxide of iron and sulphuric acid with the least possible expenditure of both, without occasioning any loss, and without giving inaccurate results.

Process adopted to save both.

After various trials, the following method appeared to him most speedy and certain. 100 grs. of pyrites, reduced to very fine powder, were diffused in half an ounce of water in a twelve ounce phial; and concentrated nitric acid was added, drop by drop, as long as a brisk effervescence took place, with the evolution of red fumes. The mixture being then exposed to a gentle heat, as soon as no farther action was produced the pyrites was completely oxided. In several trials he employed about 11 drachms of nitric acid, and the process occupied a quarter or at most half an hour. By the addition of a small quantity of water, the loss that would have been occasioned, if concentrated nitric acid had been employed, was avoided.

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* Abridged from the Annales de Chim. vol. lxviii., p. 134. Translated from the German by Berard.

† For a careful analysis by Gueniveau, where the proportions are 45 iron, 55 sulphur, see Journal, vol. xxi, p. 142. C.

1st specimen of pyrites.

The first specimen analysed was a grayish yellow pyrites perfectly crystallized in cubes. The filtered solution left 4 grains of silex; and muriate of barytes threw down a precipitate amounting to 355·5 grs. Hence Mr. Bucholz calculates the pyrites to have contained 51·15 per cent of sulphur, estimating the sulphate of barytes to contain 32·5 of acid, and sulphuric acid 42·5 of sulphur.

2d specimen.

The second specimen was a pyrites crystallized in cubes, with concave surfaces, and the edges slightly blunted. 100 grs. left 4·5 of silex, and formed 358 grs. of sulphate of barytes. This therefore, calculating on the same data, contained 51·77 per cent of sulphur.

3rd. specimen.

Mean.

The third was a pyrites crystallized in radii 100. grains left 2 of insoluble matter, and produced 352 grs. of sulphate of barytes. Hence Mr. Bucholz calculates its sulphur at 49·61 per cent; and estimates the mean proportions at 51 sulphur, 49 iron*.

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* If we take the proportion of acid in sulphate of barytes to be 33·5 per cent, and that of sulphur in the sulphuric acid to be 43·28 per cent; the proportions which Dr. Henry has adopted in his Elements of Chemistry, lately published; we shall find, that the first specimen gave 53·66 per cent of sulphur, the 2d 54·36, and the 3d 52·66; the mean of which is 53·56 to 46·44 of iron. C.

VI.

Description of Phosphated Copper: By MR. HERSART, *Mine Engineer**.

Essential characters.

PHOSPHATED copper, whatever be its form, is of a very dark or bottle green on its surface; but internally of a fine emerald green, bright and shining or mixed with reflections of black.

It is soluble without effervescence in nitric acid, to which it gives a sky blue colour, as it does to ammonia. Iron precipitates copper from the nitric solution.

Its specific gravity is 4·07031.

It is easily scraped with a knife, scratches pure carbonate of lime, and is scratched by common glass.

The powder is always of a lighter green than the mineral in substance.

In thin pieces it is translucid.

The fracture of its crystals is lamellar, that of the drusy specimens fibrous. The latter has not the brilliancy of the former, but in some specimens it exhibits a silky or satiny lustre.

Before the blowpipe the phosphate of copper fuses easily, producing first a brittle globule, dull, and of an ashen or blackish colour. If we continue to heat the globule on a piece of charcoal with the addition of any kind of grease, a small button of red copper will be obtained; but a part will still remain in the state of blackish scoriæ. This residuum dissolves in nitric acid with effervescence, giving it a sky-blue tinge.

If the phosphate of copper be fused before the blowpipe with borax, we obtain a bright red glass.

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* Abridged from the Journal des Mines, Vol. XXIV, p. 331. Phosphate of copper not being much known to mineralogists, we imagine the following description will be found interesting, as it has just been drawn up on the spot where the mineral is found, and as it differs in some respects from the descriptions hitherto published: while, having been made from a great number of specimens, it appears to us to deserve confidence. *French Ed.*

Distinguishing characters.

Phosphated copper may be distinguished

1, from green carbonate of copper, by dissolving in nitric acid without effervescence, and giving a blue colour:

2, from muriate of copper, by not giving a blue and green colour to flame, on which it is thrown, as the muriate does:

3, from arseniated copper, by not emitting an arsenical smell when acted on by fire, and giving a blue colour, instead of a green, to nitric acid.

Varieties of form.

The crystals of phosphated copper are commonly grouped so as to exhibit but one face, or one solid angle. The faces are seldom plane and smooth, being almost always curved, and subdivided into a great number of small facets with different inclinations. When the faces are smooth and plane, they appear as if striated parallel to one of the edges: and in these crystals we perceive two opposite faces, which are neither plane nor smooth, but rough and full of little points. The form of these crystals appears to be a rhomboid, approaching to a cube.

Single or detached crystals are occasionally found. These exhibit a rhomboid apparently more acute than the preceding; but neither their faces nor edges are sharp and well defined.

Sometimes it occurs in small scales, lying on each other, and inclined in different directions.

The fibrous phosphate of copper is found either in masses externally drusy; or lining cavities. This variety exhibits internally fine and close striæ, arranged in bundles of divergent radii, issuing from one or more centres. This variety has sometimes a silky or satiny lustre within.

Where found.

The phosphated copper just described is found in the bed or vein called Venusberg, or Josephberg, not above half an hour's journey from the town of Rheinbreitbach. I have said bed or vein, because, if examined in different places, it appears sometimes one, sometimes the other; and hence mineralogists are not decided which to call it. I am inclined however, to consider it as a vein, from the resemblance between this mountain and that of Mariaberg, which contains unquestionable veins of pyritous copper, and is not above 20 or 25 minutes distant from that in which the phosphated copper is found.

The vein of Josephberg is contained in a mountain, that appears to be composed entirely of argillaceous schist, or rather a schistose clay containing mica, but in particles scarcely perceptible.

Ores accompanying it.

The phosphate of copper accompanies copper pyrites, native copper, acicular or earthy oxidule of copper, and blue and green carbonates of copper. The latter is found also in the state of malachite, or compact carbonate of copper. Sulphate of copper too occurs, though very rarely, in this vein, which is very thick, and its extremity comes out to day near the summit of the mountain.

Gangue.

The gangue of these ores is commonly a white or grayish hyalin quartz, frequently tinged of a brown yellow by oxide of iron, which is likewise found uncombined in the specimens. Sometimes the hyalin quartz is tinged of a pale green by the phosphated copper.

Beside the hyalin quartz, but more rarely, a stone is found as its gangue, which the director of the works calls hornstein, but which I am inclined to consider as a true agate quartz. If it be rare to see this agate quartz form the mass of the specimens, it is frequently found lining their cavities. The surface of the phosphate of copper, particularly of the drusy, is often covered with a pellicle of common chalcedony, which is so thin, as not to be always perceptible; but if a fragment be exposed to the blowpipe, the chalcedony immediately separates from the phosphated copper, and appears with its white colour. This pellicle sometimes gives the druses the appearance of mouldiness, or renders their surface velvetty. In the cavities too, that contain the phosphated copper, a white chalcedony, or sometimes slightly tinged with blue, is found in separate and parallel cylindrical tubes, and occasionally in slender threads, crossing each other in various directions. These are either white or rose-coloured. In the axis of the tubular chalcedony we frequently find an opake line, apparently owing to the oxide of iron. Sometimes among the fibrous phosphate of copper particles of green carbonate of copper are found, which are distinguishable by their paler colour.

The preceding description differs in several respects from that of Mr. Karsten, who no doubt had but few specimens to examine. In saying, that its colour externally was grayish black, he was probably misled by some specimens covered with a pellicle of chalcedony, which in fact have this colour. Mistake of Karsten.

VII.

Comparative Analysis of Gum-Resins: By MR. HENRY BRACONNOT, *Professor of Natural History, &c.*[*]

THE substances I purpose to examine are interesting, have hitherto been considered only in a few points of view, and the labours of Boulduc, Geoffroy, Neumann, and Cartheuser, leave much to be desired with respect to them. Besides, the great progress made by chemistry since their time demands a fresh examination of the gummy-resinous substances, of which modern chemists have taken but a cursory view; and this emboldens me to consider in a new light the concrete juices of vegetables, availing myself of the present state of chemical science. If I have attempted an undertaking beyond my strength, at least I will not have to reproach myself with not having done my best to merit the approbation of the learned. At present I shall bring forward only a part of my labours, intending soon to complete them. Gum-resins a fit subject of inquiry.

ARTICLE I. *Analysis of Aloes.*

§ I. The aloes, that was the subject of this examination, was of a yellowish red, and semitransparent; in its fracture it exhibited several yellow specks shining on a red ground; reduced to powder it was of a fine yellow colour; it had a very bitter taste, and a smell not disagreeable to some people. It did not become electric by friction. Aloes described.

Exposed to a heat of 80° R. [212° F.] it first softens, and then melts. This fusibility is the cause of its being much Action of heat on it.

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[*] Abridged from Ann. de Chim. vol. LXVIII, p. 19. The paper was read to the Academy of Sciences at Nancy the 14th of Jan. 1808.

much more easily pulverable in winter than in summer. If a piece be held in the flame of a candle, it melts, swells up, and takes fire.

Distilled.

§ II. Fifty grammes [772 grs.] being distilled with a heat very gentle at first and incapable of decomposing the aloes, the products were:

Products.

1st, 8 gram. [123·5 grs.] of water, impregnated with the essential oil from which the smell is derived.

2d, at a higher degree of heat came over 8·7 gram. [134 grs.] of a nearly colourless water, in which I found some acetic acid, but no ammonia, on adding quicklime in powder.

3rd, 5 gram. [77·2 grs.] of a heavy red oil, soluble in alcohol.

4th, a large quantity of oily hidrogen gas and carbonic acid.

Coal.

5th, there remained in the retort, which had experienced a commencement of fusion, 20 gram. [308·8 grs.] of a hard coal, very bulky and swelled up, retaining a large quantity of hidrogen, which was seen to burn on exposing it a long time to a strong heat in a crucible for the purpose of incinerating it, which was found to be impossible. It retained all its blackness, its brilliancy, and considerable hardness; yet it had lost 12·5 gram, [193 grs.] of its weight, which I ascribed in great part to hidrogen. The 7·5 gram. [115·8 grs.] that remained contained no sensible quantity of potash.

Having treated this coal with muriatic acid, the filtered liquor was precipitated by ammonia, which separated some oxide of iron, and a small quantity of phosphate of lime. Carbonate of potash precipitated a few decigrammes of carbonate of lime.

On heating nitric acid on this coal a small quantity of tannin is obtained, which precipitates glue.

Aloes completely dissolved in a large quantity of cold water,

§ III. Powdered aloes, triturated in a glass mortar with cold water, yielded a substance having the tenacity of turpentine when worked between the hands. I obtained a complete solution by successive additions of water, but it required a large quantity. The last portion, that remained to be dissolved, was similar to the first in its bitterness and other properties. This solution froths when shaken.

148 gram.

but much more soluble in hot.

148 grains. [2285 grs.] of water, at 32° R. [104° F.] were sufficient for the complete solution of 4 gram. [61·8 grs.] of aloes, except a decigr. [1·5 gr.] of impure woody matter. As it cooled, the solution grew turbid, and let fall a portion of the matter dissolved. This solubility of aloes in water is so much increased by heat, that we may obtain a solution of the consistence of a sirup, which then lets fall nothing, and is even capable of crystallizing, on boiling it down still farther.

Properties of the aqueous solution.

The aqueous solution of aloes exhibited the following appearances with reagents.

1. Infusion of litmus is very perceptibly reddened by it.

2. Alkalis and lime-water render its colour deeper, without precipitating any thing.

3. Sulphate of iron produces in it a brown colour, and in a little time a precipitate of the same hue.

4. Decoction of galls produces a yellowish flocculent precipitate. The supernatant liquid is paler, and much less bitter than before.

5. The subacetate of lead likewise occasions a precipitate, and the supernatant liquid is nearly colourless.

5. Nitrate of copper and of lead, and muriate of tin, likewise occasion slight depositions; but these do not appear to be real chemical compounds, for the solutions of muriate of soda, and other neutral salts, effect as much. These saline substances therefore act on the solution of aloes in the same manner as on that of tannin in water, merely by weakening the action of this liquid on the difficultly soluble matter it contains.

Action of air on the solution.

This solution of aloes, which was of a fine gold colour, was left to settle in three vessels. The first, containing a quart, was quite filled with it, and well-corked: the second, of the same size, was but half full, and uncorked: the third, an apothecary's phial, was likewise open, and but a quarter filled. At the end of ten weeks the solution in the first retained its colour unchanged: that in the second was of a very deep red, but rendered colourless by oximuriatic acid, which produced in it a flocculent precipitate: in the third, a quantity of mucus was formed. The coloured liquids in the last two had acquired a degree of viscosity:

cosity: in fact, a matter analagous to gelatine seemed to be formed, for decoction of galls produced in them a precipitate much more copious than in the recent solution.

These facts seem to me to prove, that aloes is not a resin.

Solution of aloes in alcohol.

§ IV. Spirit of wine, at 38° [sp. grav. 0·827] dissolves aloes entirely with great readiness, particularly if hot, which indicates the absence of gummy or extractive matter. The solution, when filtered to free it from some particles of foreign matter, is of so deep a red, that it is difficult to perceive its transparency. Water produces in it a copious sediment of a pale yellow colour. This colour is owing to the water retained in it, for on drying, it assumes its original brown.

Crystallizable.

If the alcoholic solution be evaporated, towards the end we find, that the least motion, the slightest breath on it, produces a sort of crystallization, which disappears again, but is soon after reproduced.

Aloes insoluble in oils.

Though alcohol dissolves this substance very well, it is not the same with oil, either fixed or volatile. I exposed to heat a mixture of olive oil and aloes: the latter remained fused at the bottom. Oil of turpentine boiled on aloes comported itself nearly in the same manner, but it acquired a light amber tinge.

Action of alkalis on aloes.

§ V. Alkaline solutions dissolve aloes very readily without heat, and the results are combinations, in which the bitterness appears partly concealed *. Acids throw down from these solutions copious precipitates, which become coloured by desiccation. Volatile alkali diluted with water likewise dissolves aloes completely. The filtered solution was of a deep red, and was evaporated slowly, to expel the excess of ammonia. As the solution was concentrating, a continual movement appeared on the surface, seeming to indicate a tendency to crystallization, for needles were observed appearing and disappearing in succession. On continuing the evaporation almost to dryness, needly crystals were obtained, imbedded in a kind of resiniform mass. On

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* The mixture of seven drachms of tincture of aloes with one drachm of the liquid subcarbonate of potash has the taste of a solution of extract of liquorice very nearly. C.

heating this with a certain quantity of lime and water, ammonia was very perceptibly evolved.

Action of acid on aloes.

§ VI. Weak acids have no very striking action on aloes, yet they dissolve it better than water, which whitens the solution of aloes in distilled vinegar. The mineral acids act on it much more powerfully. Nitric acid dissolves it very well without heat, producing a deep red liquid, which water precipitates abundantly.

Nitric acid.

Ten gram. [154·4 grs.] of aloes were treated in a retort with 80 gram. [1235 grs. of nitric acid at 36°, taking care to raise the fire cautiously. The action was brisk, and abundance of red fumes were evolved. When these disappeared, the retort was removed from the fire. The liquid contained in it was of a deep yellow colour, and on cooling deposited a pretty large quantity of a yellow flocculent substance. Being evaporated to the consistence of honey, it was diluted with water, and filtered. A yellow substance remained on the filter, which, after having been well washed and dried, amounted to about a fourth of the aloes employed. This appeared to be an acid, analogous to the yellow, acid, and detonating matter which Fourcroy and Vauquelin obtained by the action of nitric acid and animal substances, but differing slightly in several respects.

Yellow acid produced.

Its properties.

The yellow aloetic acid, well washed and dried, is of a fine yellow colour, and extremely bitter. It does not crystallize. It reddens litmus paper, and effervesces with alkaline carbonates. It has a pleasant aromatic smell, particularly when gently heated. It melts like nitre, emits an aromatic vapour mixed with bitterness, and leaves an abundant coally residuum. Distilled with a gentle heat, it furnished all the products of vegetable substances, and finally detonated with a purplish flame. A very bulky coal remained, equal to a third of the matter employed.

This acid is very little soluble in water. It required 1250 times its weight of water at 10° R. [54·5 F.] for its complete solution. This was of the fine red colour of arterial blood. Muriate of tin produced in it a precipitate of the colour of wine lees. The sulphates of iron and of copper brightened the colour.

Alcohol at 38° dissolved only one thirtieth its weight of the yellow acid. The solution was a very deep red.

Hot mineral acids dissolve this yellow matter without evolving any thing; but it is soon after deposited, in consequence of its insolubility.

Neutralized with potash detonates.

Potash forms with it a compound of a deep red, and capable of crystallizing. This red salt detonates with the violence of gunpowder, either when exposed to a certain degree of heat, or touched with a burning coal; and after burning leaves a slight coally trace, and a remarkable smell of prussic acid, which leads to a suspicion of the presence of nitrogen.

This red detonating substance is easily produced by pouring on the yellow acid of aloes a weak hot solution of caustic potash, which has but a slight solvent action on it.

Nitric solution.

The nitric solution, from which the yellow aloetic acid has been separated, was saturated by potash. At the end of twenty-four hours a very small quantity of red detonating matter was deposited. Nitrate of lime being added to it, a copious precipitate of oxalite of lime took place, which, when well washed and dried, weighed 3·5 gram. [54 grs.] The liquid separated from the oxalate of lime was precipitated by nitrate of lead; and the precipitate, treated with a third of its weight of dilute sulphuric acid, yielded about a gramme [15·4 grs.] of malic acid partly dried.

Not a gum-resin,

§ VII. From these facts it follows, that aloes is not a gum-resin, as has been supposed, since neither of these two principles is found combined in it. Consequently too it cannot be confounded with the resins, though it is more similar to them than to the gums. It is therefore a principle *sui generis*, which from its properties I would call *resinoamer*. This immediate principle is probably very common, and has its species, like other vegetable matters. It is this, that was at first confounded with resins, that has been sometimes taken for oxigenated extract, and that Mr. Vauquelin has made known in his interesting paper on the different species of cinchona*. It is the same substance too, that is deposited in greater or less quantity from the

but a simple substance,

occurring in other plants.

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* See Journal, vol. xix, p. 106, 203.

decoctions of several bitter plants of the class syngenesia, in which febrifuge virtues have long been acknowledged, as wormwood, centaurea calcitrapa and benedicta, succory, dandelion, and likewise fumitory*. It is true, that these plants have been found less efficacious than the astringent febrifuges; and I am persuaded, that the principle in cinchona, which acts specifically against fever, and the periodicalness of diseases, is owing to the combination of the resinoamer with tannin, or a similar matter. Following these ideas, my colleague, Dr. Haldat, intends to make some important experiments, that may lead to great and useful discoveries, and of which he will give an account.

Medical properties of aloes.

Galls destroy its purgative properties.

We know that aloes, taken internally, is a very active tonic, and externally is a very powerful antiseptic. Would it have this antiseptic power internally? It is likewise acknowledged, to have febrifuge and purgative properties: but it is not commonly known, that it ceases to purge when mixed with powdered galls, a fact I have found by experience.

REMARKS.

Other analyses of aloes.

TO complete this examination of a valuable drug, pretty extensively used in physic, before we proceed with prof. Braconnot to other gum-resins, we shall give an abstract of the analyses of it by Trommsdorff and by Bouillon-Lagrange and Vogel, both in the same volume of the Ann. de Chimie, that by Trommsdorff being taken from his Journal of Pharmacy. The following are the results of Mr. Trommsdorff's analysis.

Results of Trommsdorff's analysis.

1. *Succotrine aloes* dissolves entirely in boiling water; but the resinous part separates on cooling.

2. It dissolves also in alcohol without leaving any residuum.

3. The parts soluble in water contain more bitter principle than those soluble in alcohol, though the latter are not destitute of it.

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* It appears to me, that the resiniform matter found in bile by Thenard has a great deal of similarity with the resinoamer of aloes.

4. The *hepatic* differs from the succotrine aloes in containing some albuminous animal matter, and less resin.

5. It does not dissolve completely in boiling water, because the heat coagulates the albumen.

6. Neither is it totally soluble in alcohol. This readily distinguishes it from succotrine aloes.

7. The saponaceous principle* and resin appear to be of the same nature in both kinds.

Component parts of succotrine and hepatic aloes.

8. Succotrine aloes consists of 75 parts of bitter saponaceous principle, 25 parts resin, and a trace of gallic acid.

9. Hepatic aloes contain 81·25 saponaceous principle, 6·25 of resin, 12·5 of albumen, and a trace of gallic acid.

Bouillon-Lagrange and Vogel's analysis by dry distillation,

Messrs. Lagrange and Vogel experimented on much larger quantities than either Braconnot or Trommsdorff. They distilled a kilogramme [near 2¼ lbs avoird.] of each kind in a large glass retort. Toward the end of the process a shining black substance sublimed, which was nothing but aloes. The water from the dry distillation of the hepatic aloes they say was perceptibly ammoniacal; that from the succotrine merely exhibited a white vapour with muriatic acid, after the addition of a little pure potash.

and by wet.

Succotrine aloes.

They afterward distilled a similar quantity of each, previously diluted with a quart of water. The succotrine aloes yielded a liquid not acid, of a very sweet and pleasing smell, on which floated a volatile oil of a greenish yellow colour, and smelling like that of melilot. It also contained some other substance, as Trommsdorff observed, for after some time it grew cloudy.

Hepatic.

The water from the hepatic aloes was not pleasing to the smell; but rather nauseous, approaching a little to that of prussic acid. There was no trace of oil on its surface or in solution.

Saponaceous principle of plants.

* The author having dissolved a portion of aloes by boiling in 12 parts of water, a fourth part of the aloes separated on cooling. The aqueous solution being evaporated to dryness, a bitter substance resembling aloes remained, which was completely soluble in alcohol, but altogether insoluble in ether. Hence he supposes it to be the saponaceous principle of Hermbstaedt, *seifenstoff*, or *pflanzenseif*, which is thus characterized, and occurs in various vegetables, as saffron, rhubarb, &c. He supposes there are different species of it, more or less bitter to the taste.

They did not find aloes soluble in cold water like Mr. Braconnot. On a quantity of succotrine aloes in powder they poured water at 8° R. [50°F], assisting its action by frequent stirring. The clear supernatant liquor, after settling, was decanted off, and another quantity of water poured on the residuum. This was repeated, till the water, after standing on the residuum four and twenty hours, was found destitute both of taste and colour. The glutinous matter remaining was then worked between the fingers under a stream of water.

Only in part dissolved by cold water.

The first liquor poured off was very brown, and strongly impregnated with the aloes; the second and third were much less so, the rest growing weaker in succession, till the last was clear water. When the aloes had been sufficiently washed, and thus exhausted by water at 8° [50°F.], there remained a soft grayish mass, very elastic, which, when wet with water, did not stick to the fingers.

Insoluble part.

The aqueous solution of aloes, as Trommsdorff observed, evaporated gently to dryness, leaves a substance soluble in water and alcohol, but scarcely at all in ether. The resinous matter of aloes, on the contrary, is soluble in alcohol and in ether, but not in water at 10° [54·5°F.] The former dissolves readily in cold nitric acid at 36°, and forms a green liquid, which is scarcely rendered turbid on the addition of a little water, and becomes perfectly clear when farther diluted. The resinous part is more difficultly acted on by this acid, and produces a red solution, which, though much weaker than the former, throws down a resinous, sticky, insoluble substance, on the addition of a little water.

Two different substances in aloes.

Nitric acid heated on aloes produced a fine yellow powder, and nearly the same phenomena as those observed by Mr. Braconnot. This powder, diffused in a little water, communicated to it a superb purple, very rich in colour. A single atom will tinge a very large quantity of water. This colour is so permanent, that the skin remains dyed with it for several days, particularly if an alkaline salifiable base have been previously added to the powder.

Action of nitric acid.

Fine purple dye.

Messrs. B. L. and V. likewise passed a current of oximuriatic acid gas into a concentrated solution of aloes in cold

Action of oximuriatic acid

gas on the part soluble in water

cold water. A large quantity of the gas was absorbed, and the solution became yellow, and coagulated like animal jelly, so as to become almost one entire mass. The coagulum, when separated, was of a whitish yellow, but soon turned brown. After being washed, it was very elastic, insoluble in water at 8° R. [50°F], but very readily soluble in alcohol, and this solution was copiously precipitated by water. The oximuriatic acid gas therefore appears in some sort to have resinified the portion of aloes soluble in water.

converted it into a kind of resin.

Component parts of aloes.

According to them, succotrine aloes consists of 68 parts extract, and 32 resin: and hepatic aloes is composed of 52 extract, 42 resin, and 6 insoluble matter, which Trommsdorff calls albumen.

(To be continued.)

VIII.

Communications concerning the Royal Botanical Garden at St. Vincent, from its Superintendant DR. ALEXANDER ANDERSON, *to* DR. C. TAYLOR [*].

DEAR SIR,

I Am honoured with your letter of the 26th and 28th of April, with the 21st and 24th volumes of the Transactions of the Society of Arts; also the publication on the Culture of Black Pepper, for which I feel great obligation to the Society.

Cultivation of the pepper plant at St. Vincent's.

From Mr. Martyn's account of the pepper plant, I am in hopes that it will succeed in this garden; as he says it is three or four years before they produce in the East-Indies after planting, and it is now near that time since I procured them, and there are several very luxuriant at present [†]. I am happy to find I had adopted the mode of planting them which he has described.

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[*] Trans. of the Soc. of Arts, vol. xxvi, p. 234.

Success of the black pepper plant in the West Indies.

[†] In a subsequent letter, dated June the 19th, 1809, the Dr. says: "I have the pleasure to inform the Society, that the black pepper plant thrives remarkably well in this garden, and has been producing fruit more than a year. Some of its produce I now transmit to

In the West Indies East India plants thrive, Chinese do not.

In general I find that East-India plants are more rapid in their growth, either from seeds or plants, than the indigenous plants of the country, and arrive at perfection sooner; but the reverse is the case with the Chinese. There is at present in the garden a large tree of the *litche*, sent by sir Joseph Banks in 1788, which as yet has made no attempt to flower. I experience the same disposition in several herbaceous perennial plants from China. I was pleased to see a specification of growth of trees in the East Indies, by Dr. Roxburgh, in the last volume of the Transactions, which led me to a comparison of some East Indian trees here, and also of some natives; and I find those from India thrive full as well here as in their native soil. The result I send you. It is a matter of curiosity, rather than utility. It shows the rapid progress of vegetation in tropical climates, compared with that in the colder regions.

Many natural productions neglected for the sugarcane.

Of the numberless articles for commerce and economy, manufactured in the East-Indies, no attention is paid to them here, although many of them are common. The same is the case as to small products for necessary existence. This is owing to the want of a proper population, and the high price of manual labour. Except in Barbadoes, and a few other islands, all the land in cultivation is engrossed by the sugar cane. No room is left for poor industrious people, unless in detached spots remote from towns, markets, and shipping. The hard woods fit for mill timber are more attended to than any other, and they undoubtedly are the most essential article to the planters, yet few take the trouble to plant them, or give room for them.

Tan to be had in the West Indies.

You mention the high price of oak bark for tanning. I am confident we have many barks here superior to it, as to the astringent principle. Whether our barks are as effectual, or more so, than the oak bark in tanning, deserves ex-

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to you for the Society's inspection. The berries are collected before full maturity. I find it is a plant of more easy cultivation than I conjectured. After it begins to bear there is no intermission. It yields its berries in succession during the year. As soon as one crop comes to maturity, the plant recommences flowering.

periment. For that end I will transmit you some specimens by the first opportunity. The barks might be imported at a trifling expence, unless the customhouse duties should be found to prevent them. The high duties and prohibitions in the customhouse prevent several people here from sending similar articles hence, for experiments, as well as for speculation in trade. A few persons in this island wish to cultivate the cinnamon for commerce; they have asked me if it can be entered at the customhouse, and what are the duties upon it? I could give them no information as to either. The overhauling and pilfering, by the customhouse officers in England, of articles of natural history, sent as specimens, is very injurious; such things should be held sacred.

Obstacles at the customhouse.

Many articles here deserve to be subjected to experiments, from which I am prevented by the necessary attentions to the garden, particularly for some time past. The business of it engrosses all my time and care, and is as much as one individual can attend to.

I will endeavour to obtain a correspondence with Dr. Roxburgh, but I almost despair of it during the war. I have correspondents in America, whom I can depend upon;* but the conveyance, through the medium of American vessels to these islands, is very precarious. Some time ago I lost a parcel of seeds from New-York, sent in charge by one of these vessels. I believe I mentioned, that I have lost one of my nutmeg plants, for which I blame myself, by too much attention in watering it in dry weather. The other thrives remarkably well, and is now above ten feet high; but if it proves a male, I am at a stand. Could I find the opportunity of sending by a flag of truce to Cayenne, I know I could get a supply.

Correspondence interrupted by the war.

Nutmeg killed by overwatering.

I am, with most sincere regard,

Dear Sir,

Your much obliged and ever grateful Servant,

ALEXANDER ANDERSON.

Botanical Garden, St. Vincent, July 21, 1807.

DEAR SIR,

I Have the pleasure to inform you, that some of the black pepper plants are now pushing out freely their fructification; but have to lament, that the only nutmeg in

Black pepper.

Nutmeg.

in the garden proves a male, and there is no prospect, at present, of obtaining more, as in the present situation of affairs, no communication from St. Vincent to Cayenne can be had by flags of truce or otherwise. Several plants of it were brought to Trinidad with the colony of the Chinese: I much fear they are, or will be lost there.

Chinese colony at Trinadad.

Cloves.

I send you some cloves, about two thirds of the produce of one young tree for the first time. My reason for troubling the Society with them is from a wish to know whether drying them in the shade or sun is the most proper mode, or if it makes any difference in the quality of the spice; if not, they may be cured in the sun with no trouble, in a very short time. The young fruit I reserved on the tree for seed, part of which was beaten off by the wind, and seems to me little inferior to the flower buds.

Substitutes for hemp.

Fibres of the aloe.

On reading, in the Society's Transactions, Dr. Roxburgh's Experiments on the Comparative Qualities of Bark of East India Plants as Substitutes for European Hemp*, I was induced to try the leaves of the *agave*, to ascertain how far the fibres of them would answer the purpose. I transmit a specimen of them for the society's inspection. The small bundle, tied with some twine made of the same, is the produce of one moderate sized leaf, and was obtained from it, immediately cut from the plant, in a very short time. The operation was performed by a black boy. The plants are produced in abundance by nature among the rocks by the seaside and barren hills. If found useful, any quantity may be obtained with little labour and no expense of first cost. The superior advantage over the East India articles (most of them common in these islands) is the trifling labour requisite to get the fibres from the fleshy substance of the leaves without steeping, or any other previous process. When macerated in water, I think it lessens the strength of the fibre, and gives it a dusky hue.

Much valued by the Mexicans.

The three small bundles, which I now send, were taken from two species common in St. Vincent, viz. *agave vivipara*, and *a. cubensis*. The leaves of all the tropical species possess much the same properties. By the ancient Mexicans, the agave was deemed the most valuable production of nature. It is mentioned by all the Spanish writers on America under the name *maguei*.

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* See Journal, vol. xi, p. 32, and xvi, p. 223.

Maguei of the Spaniards.

Nothing attended to but sugar.

It is to be observed, that no article in these islands, however valuable, and whatever encouragement may be held out for its manufacture, will be attended to in their present situation. The sugar cane is considered as the only plant, that merits the attention of the planters.

Substitutes for oak bark.

In my last to you I mentioned barks of trees in these islands, which I conceived may become substitutes for oak bark in tanning. I transmit you specimens from five different trees, which are all common, and consequently readily procured, if they prove useful. That of the *maljughra* I know the Spaniards use on the main land with that intention. The quantity of each is purposely small, for the more easy conveyance, and prevention of difficulties at the customhouse. However, they may be sufficient for ascertaining their astringent or tanning principle.

Grains of Paradise.

East India seeds.

In consequence of the war cutting off most of my opportunities of correspondence, the additions to the garden are much less, than otherwise they would have been; however, almost every day some thing or other is obtained from some parts of the world. What I have long wished for, the grains of Paradise, are thriving luxuriantly. By the last fleet a number of East India seeds arrived; many of them will be valuable acquisitions, if they vegetate.

I am, with the greatest regard,
Dear Sir,
Your much obliged,
Most humble and obedient Servant,
ALEXANDER ANDERSON.

St. Vincent, Botanical Garden,
April 16, 1808.

Table

Table of the Growth of certain Trees in the Botanical Garden at St. Vincent.

Growth of trees in the botanical garden at St. Vincent's.

Tectona grandis—The seeds lie in the ground from eighteen months to two years, before they vegetate. They have produced seeds in the garden ten years ago.

			ft.	in.
First seeds received from Sir J. Banks in	1788			
Circumference of stem, in	1807	at 6 ft. above ground	4	6
Caryota urena, seeds from Sir J. Banks,	1792	Do.	4	10
Sapindus edulis, (Litche) plant from ditto	1788	Do.	4	8
Mimosa Lebbeck, seeds do.	1792	Do.	4	5
Sterculia fœtida, do. do.	1792	Do.	6	0
Gomutu Palm, seeds from Bd. of Agri.	1800	Do.	5	7
Artocarpus incisus, small plants -	1793	Do.	6	1
Integrifolius, do. -	1793	Do.	5	6
Jambolifera pedunculata, do. -	1793	Do.	5	9½
Aleurites triloba, seeds - -	1793	Do.	4	8
Eugenia Malaccensis, small plants -	1793	Do.	3	10½
Mangifera indica, from seeds -	1788	Do.	7	0
Ditto, small plants from E. I. -	1793	Do.	5	2
NATIVES.				
Swietenia Mahagoni, seeds has been producing plenty of seeds for several years	1790	Do.	5	4
Copifra officinalis, seeds from the Continent	1790	Do.	3	2
One of the most valuable woods.				
Mimosa grandis, seedling plant from the Continent	1792	Do.	6	6
A very hard and valuable wood.				
Carolinia insignis, seeds from Trinidad	1787	Do.	8	0
The wood of no value.				

St. Vincent, July 21, 1807.

A. ANDERSON.

IX.

On the Oxides of Iron. By THOMAS THOMSON, *M. D. F. R. S. E. Fellow of the Imper. Chudrurgo-Med. Acad. of Petersburg.*

Remarks on Dr. Thomson's Chemistry by Hassenfratz.

IN the Annales de Chimie for May 1809 (vol. lxx, p. 145) there is an article by Mr. Hassenfratz, of which the following is an abstract.

"I have just received a copy of Thomson's System of Chemistry translated by Riffaut. I opened the first volume, and read with eagerness the tenth section, which treats of iron. The details published in that section were the more interesting to me, as I have been for these two years employed by the minister of the Interior, to describe the art of extracting iron from its ores, and to explain the different operations, which it undergoes before it is brought into the commercial world in the states of cast iron, iron, and steel. You may guess my astonishment, when I read the following passage. '*The peroxide of iron is also found native in great abundance. Proust proved it to be composed of 48 parts of oxigen and 52 of iron. Consequently the protoxide, when converted into red oxide absorbs* 0·40 *of oxigen; or, which is the same thing, the red oxide is composed of* 66·5 *parts of black oxide, and* 33·5 *parts of oxigen. One hundred parts of iron, when converted into a protoxide, absorb* 37 *parts of oxigen, and the oxide weighs* 137; *when converted into peroxide, it absorbs* 55 *additional parts of oxigen, and the oxide weighs* 192·3.

Proust's account of the oxides of iron.

"Proust has not said, in any work that I know, that the red oxide is composed of 48 parts of oxigen and 52 of iron. What may have led Dr. Thomson into errour is, that in the memoir of the celebrated chemist of Madrid, published in vol. xxiii, p. 65, of the *Annales de Chimie*, it is stated, that he announces the existence of the two oxides of iron, the one at $\frac{27}{100}$ of oxigen, the other at $\frac{48}{100}$. As it is not said in any article of the memoir, whether the 48 of oxigen were in the 100 of oxide, or combined with 100 of metal, this manner of expressing the proportion of oxigen has left a kind of uncertainty in the minds of those chemists, who have made no experiments on the proportion of oxigen in the oxide of iron. The learned British chemist, who certainly has made no experiment to resolve the question, has adopted the simplest meaning of the fraction $\frac{48}{100}$; and this has occasioned the errour in the passage, which I have quoted;"

Usual meaning of his fractional expression.

Mr. Hassenfratz then proceeds to show, that in other parts of his writings Proust is in the habit of denoting by the numerator of his fraction the quantity of oxigen, and by the denominator the quantity of metal: of course $\frac{48}{100}$ mean an oxide composed of 100 iron and 48 oxigen. He then proceeds to point out the true composition of the oxides of iron, and thus to correct the above passage in my work. But it is not necessary to transcribe the rest of his paper, as he had already published an elaborate dissertation on the subject in the lxix volume of the *Annales de Chimie*, in which the subject is much more fully discussed; and to which therefore I refer the reader*.

Experiments instituted to ascertain the true proportions in oxides of iron.

The perusal of Mr. Hassenfratz's paper, while it convinced me of the mistake into which I had fallen, induced me to make some experiments on the composition of the oxides of iron, in order to verify and establish the proportions obtained by others. My object at present is to state the results which I obtained.

Red oxide.

Two methods employed.

I. The red oxide of iron, or the oxide containing a maximum of oxigen, is too well known to require a particular description here. Two methods have been followed by chemists, to ascertain the proportion of oxigen which it contains. The first is to expose a determinate weight of iron to a red heat, triturating it occasionally, till it ceases to acquire any additional weight. The second is to dissolve iron in acids, and to expose the salt obtained to a heat sufficiently high to decompose it. The red oxide remains, and its weight gives the addition, which the iron has acquired by its oxidizement.

Iron filings calcined.

The first method appears at first sight easy, but it is in reality exceedingly difficult. Accordingly the experiments of Scheffer, Morveau, Lavoisier, Darso, Bucholz, and Hassenfratz differ so much from each other, that no satisfactory conclusion can be drawn from them. I consider the experiment of Hassenfratz as the most accurate. 100 parts of iron in his trial were converted into 145 of red oxide †. In Darso's experiment 100 parts of iron were augmented to 156 of red oxide ‡. But as this greatly exceeds what was obtained by every other person, we must suppose a mistake. I have not tried this method, being deterred by its uncertainty.

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* A translation of this paper is intended for insertion in this Journal at an early opportunity. A shorter paper of Hassenfratz on the same subject occurs in vol. xxvi, p. 47. C.

† Ann. de Chim. vol. lxvii, p. 309. Journal, vol. xxvi, p. 147.

‡ Journal de Phys. 1809 tom. ii, p. 294. Journal vol. xvii, p. 224.

Difficult to find iron perfectly pure.

Specimens analysed.

Polished iron wire best.

The second method is easier, and more satisfactory. The greatest difficulty, to which it is liable, is that of procuring iron in a state of absolute purity, to make experiments upon. I have tried many varieties, and have applied to those artists, who were likely to have iron in the greatest purity. But hitherto I have not been lucky enough to find a single specimen absolutely pure. I was obliged therefore to analyse the specimens which I employed, and to make allowance for the impurities, which varied in different specimens from $\frac{1}{100}$th to $\frac{1}{1000}$th part of the whole. Polished iron wire is most convenient. Iron filings, unless made on purpose, are not sufficiently pure, and it is more difficult to dissolve them completely than iron wire.

Dissolved in nitric acid.

Reduced to red oxide

gained near 45 per cent.

100 grains of iron wire were dissolved in diluted nitric acid. The solution goes on rapidly, and is at first opake, and almost black, owing to the nitrous gas which it retains. This gas gradually separates, and then the liquid is nearly colourless. When concentrated it becomes of a brownish yellow colour. It was evaporated to dryness, and exposed for a quarter of an hour to a red heat in a platinum crucible. The red oxide thus obtained weighed 142·6 grains. In another experiment made in the same way 100 grains of iron were converted into 144·75 of red oxide. This last result I consider as the most correct, because it coincides nearly with the result obtained by Hassenfratz in a different manner, and because in experiments of this nature, where liquids are evaporated to dryness, there is always a risk of some loss during the evaporation. On this account, in making choice of various results, that which gives the greatest weight has the most chance of being correct. Upon the whole then we may conclude with considerable probability, that the red oxide of iron is composed of 100 iron and 45 oxigen.

It could not be deoxidated by heat alone,

I tried to deprive the red oxide of iron of part of its oxigen by various methods, but without success. No degree of heat, which I could raise, was capable of disengaging oxigen gas from it, though the oxide acquired a black colour. When the red oxide is mixed with oil, and heated to redness, it becomes black, and is attracted by the magnet: but its weight is not altered. Indeed, if we repeat the experiment a great number of times with the same portion of oxide, the weight rather increases. When red oxide is heated with charcoal, it is reduced to the metallic state.

but with the addition of oil became magnetic; though without any alteration in weight. With charcoal reduced.

Iron dissolved in sulphuric acid.

When iron is dissolved in sulphuric acid, the solution evaporated to dryness and exposed to a strong heat, the sulphuric acid is dissipated, and red oxide of iron obtained. But experiments made in this way do not lead to a satisfactory result. 100 parts of iron thus treated were converted into 150 parts of red oxide. But it was not quite pure, still containing traces of sulphuric acid. This was the case even when the oxide had been exposed to a heat sufficient to calcine carbonate of lime. The results were not more satisfactory, when the iron was precipitated from sulphuric acid by an alkali. The oxide obtained, though carefully edulcorated, still contained sulphuric acid. For when dissolved in muriatic acid, and mixed with muriate of barytes, a white insoluble precipitate fell.

Results unsatisfactory from the retention of acid.

Difficult to ascertain the proportions of black oxide.

II. To ascertain the proportion of oxigen in the black oxide of iron is a more difficult task. I shall relate the experiments which I made in order to determine the point.

Iron dissolved in dilute sulphuric acid.

1. When 100 grains of iron are dissolved in diluted sulphuric acid, the hidrogen gas produced amounts to 163·4 cubic inches, at the temperature of 60°, and when the barometer stands at 30 inches. Two experiments were made, each of which gave exactly the same result. Now it is well known, that when iron is dissolved in this manner, it is converted into black oxide. Water is decomposed, the hidrogen of which escapes in the form of gas, while the oxigen unites with the iron. It has been established, that the constituents of water, reduced to the gaseous state, consist of 2 parts by bulk of hidrogen and 1 part of oxigen. Hence in this case the oxigen, which combined with the 100 grains of iron, and converted it into black oxide, is equivalent to 81·7 cubic inches. Now 81·7 cubic inches of oxigen gas weigh, according to the experiments of Lavoisier and Davy, 27·93 grains; according to those of Allen and

Pepys 27·63 grains. The above experiments of mine were made at the temperature of 45°. If the vapour of water be subtracted according to Mr. Dalton's formula, it will diminish the weight of the oxigen about one third of a grain. It follows pretty nearly from these data, that black oxide of iron is composed of 100 parts by weight of iron and 27·5 of oxigen. Bergman, Berthollet, Vandermonde, and Monge made many experiments on the quantity of hidrogen gas given out, when iron is dissolved in diluted sulphuric acid; but their results differ so much among themselves, owing probably to the great difference in the purity of the different specimens of iron employed, that no satisfactory consequences can be deduced from them.

Black oxide contains 27·5 of oxigen to 100 metal.

2. When iron wire is burnt in oxigen gas, it is converted into black oxide. Mr. Lavoisier made many experiments on this combustion, from which he concluded, that 100 parts of iron combine with between 32 and 35 parts of oxigen*. I repeated this experiment several times, with every possible precaution to insure accuracy. All the trials corresponded so nearly, that it will be only necessary to state one of them. 11·81 grains of iron wire were burnt in oxigen gas. The black oxide formed weighed 15·01 grains. Hence 100 parts of iron would by this process have been converted into 127·09 grains. This result agrees nearly with the preceding. The proportion of oxigen, which appears to combine with the iron, is indeed a little lower. But the reason I believe to be, that, during the combustion of the iron, small particles of it are dissipated in sparks, which cannot afterward be collected and weighed. This quantity is indeed very minute; but still it is something, and may be seen very well, when we examine the cloth upon which the oxide is washed. If it amounted in the preceding experiment to the 20th part of a grain, it would bring up the proportion of oxigen to 27·5, the same which was deduced from the hidrogen gas emitted during the solution of iron in diluted sulphuric acid.

Iron wire burned in oxigen gas.

The result agreed with the preceding.

3. When black oxide of iron is dissolved in nitric acid, the solution evaporated to dryness, and the dry mass exposed

The black oxide dissolved in

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* Annales de Chimie, tom. I, 19.

nitric acid and converted into red.

This experiment difficult.

The most accurate

gave similar results with the preceding.

to a red heat, it is converted into red oxide. This furnishes us with another method of estimating the quantity of oxigen in black oxide of iron. Bucholz had recourse to it, and found, that 100 parts of black oxide are by this treatment converted into 110 of red oxide*. On repeating the experiment, I found it attended with more difficulty than I expected. It is not easy to procure black oxide in a state of purity. My first trials differed so much from each other, that I was obliged to conclude, that my black oxide contained some red oxide mixed with it. Another difficulty is to dissolve black oxide of iron in nitric acid. It resists the action of that acid with great obstinacy, even when in the state of a fine powder. After repeated failures, I at last succeeded in obtaining results, which agreed with each other. The following I consider as the most accurate of these. 16·77 grains of pure black oxide were dissolved in nitric acid. The solution was evaporated to dryness, and the dry mass exposed to a red heat, in a platinum crucible. It weighed 19·1 grains. Hence 100 grains of black oxide by this treatment would have been converted into 113·89 grains of red oxide. Now if red oxide be a compound of 100 metal and 45 oxigen, it is obvious, that 113·89 grains of red oxide contain 78·5 grains of metal, therefore 100 parts of black oxide are composed of 78·5 metal and 21·5 oxigen, or the oxide consists of 100 metal combined with 27 oxigen—a result which agrees very nearly with that deduced from the two proceeding sets of experiments.

Iron wire converted into black oxide by steam.

Results unsatisfactory.

4. I introduced 300 grains of polished iron wire into a porcelain tube, placed the tube in a furnace horizontally, heated it to redness, and then caused a current of steam to pass through it for several hours. By this process it is well known that the iron is converted into black oxide, while hidrogen gas is evolved in abundance. The evolution of this gas is accounted for by the decomposition of the steam. The oxigen is conceived to unite with the iron, while the hidrogen passes off in the form of gas. By this method I expected to be able to ascertain directly the increase of weight, which takes place when iron is converted into black oxide. But I was disappointed. Though the experiments

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* Journal, vol. xxv, p. 354.

One experiment described.

were made with great care, they presented anomalies, which it was impossible to reconcile with the opinions at present received. I shall describe one of my experiments particularly. Of the 300 grains of iron introduced, 63·37 grains were still found in the state of iron at the end of the experiment. The surface indeed had lost its lustre, but the malleability and other qualities remained. The specific gravity of the black oxide formed was 5·025, which agrees nearly with that of specular iron ore. The hidrogen gas evolved, reduced to the temp. of 60°, barometer 30 inches, measured 415·5 cubic inches. Hence the oxigen, which combined with the 236·63 grains of iron that had been converted into black oxide, must have been equivalent to 207·75 cubic inches, or 69 grains nearly. But if 236·63 grains of iron combine with 69 grains of oxigen to be converted into black oxide, it is obvious, that 100 grains would have combined with 29·1 grains of oxigen. This is a greater proportion than results from the preceding experiments; but the apparent differences was probably owing to the surface of the wire, which still retained its ductility, being oxidized. Were we to suppose 14·3 grains of that portion to be oxidized (and some of it certainly was, as it had all lost its lustre) it would reconcile this experiment with the preceding.

Increase of weight above that of the oxigen expended.

Was this owing to water combined with the iron?

But if the 236·63 grains of iron had combined with 69 grains of oxigen, they ought to have weighed 305·63 grains. But the actual weight was found to be 330·68 grains, or 25 grains heavier than they ought to have been from theory. This increase of weight, which was constant in all my trials, cannot be accounted for on the present universally received chemical theory; unless we suppose, that a little water, as well as oxigen, has actually combined with the iron—a supposition which was strenuously maintained by Dr. Priestley. I attempted to ascertain exactly how much of the water had disappeared in a similar experiment, but the apparatus used was so bulky, that I could not weigh it with sufficient precision, to determine so delicate a point.

5. From the whole of these experiments it seems to follow, that black oxide of iron is composed of 100 parts of metal and about 27·5 of oxigen.

Thenard's white oxide of iron,

III. When iron is dissolved in diluted sulphuric acid, if it be precipitated by an alkali, a white powder falls, which

Thenard considers as a peculiar oxide. According to him there are three oxides of iron, the *white*, the *green*, and the *red* *. I prepared a quantity of this supposed *white* oxide with all the requisite precautions, but on attempting to dry it, the colour soon changed. It became first green, then black, and last of all red. 100 grains of iron treated in this way were converted into 158·4 grains of a red powder, which lost no weight in a red heat. This red powder contained a good deal of sulphuric acid; for, when dissolved in muriatic acid, muriate of barytes threw down a copious white precipitate. Hence it is obvious, that the supposed white oxide is a subsulphate of iron. In my experiment the quantity of sulphuric acid present was about 13·4 grains. If sulphate of iron reduced to powder be digested in alcohol, it is converted into a similar white subsulphate.

a subsulphate.

Supposed native protoxide.

IV. In some of my experiments on the ores of iron, the result which I obtained did not correspond with the notion which I entertained of the composition of black oxide of iron: the oxide examined contained less oxigen. Hence I concluded, that there was an oxide of iron in nature containing less oxigen than black oxide. But it is obvious, that what I at that time considered as a new oxide is in reality black oxide, and that my black oxide was in reality a mixture of the black and the red. I allude to my analysis of iserine and of iron sand; published some time ago in the 6th volume of the Transactions of the Royal Society of Edinburgh†.

Strictures on the author's nomenclature by Hassenfratz

V. I know not whether I ought to notice a remark, with which Mr. Hassenfratz concludes the paper quoted in the beginning of this dissertation. "Dr. Thomson," says he, "or his translator, employs in the passage above quoted two new words; 1. *protoxide* to signify the oxide with a minimum of oxigen; 2. *peroxide* for the oxide with a maximum of oxigen." Fourcroy and Hauy, he tells us, had already used the word *oxidule* to denote the black oxide. He then proceeds to explain the etymology of the two terms which I employ. The Greek numeral πρῶτος prefixed to

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[*] Journal, vol. xiv, p. 224.

[†] See Journal, vol. xxviii, p. 19.

oxide, he says, constitutes the first; and the Greek preposition πηρα prefixed constitutes the second. Had Mr. Hassenfratz taken the trouble to consult my work, volume 1, p. 140, answered. (3d. edition) he would have seen, that *peroxide* was formed by joining the Latin preposition *per* to the word *oxide;* and that, according to a very common use of that preposition in composition, the word *peroxide* means a metal thoroughly oxidized, or saturated with oxigen. He then proposes to substitute for the words *protoxide* and *peroxide* the words *microsoxide* and *megaloxide*, which he says are much more precise. I believe it to be unnecessary to make any observations on this proposed substitution. In what respect these words are more precise than mine, or indeed so precise, I am at a loss to conceive. They signify literally *little oxide* and *great oxide*, phrases which lead us rather to attend to the bulk of the substances, than to the proportion of oxigen which they [illegible] But even supposing them equally or even more precise, still they could not be substituted for mine; because we require a method of naming all the oxides of a metal, even when they exceed two. My method supplies such a nomenclature; but Mr. Hassenfratz's method, even if we were to introduce also his words *oxidule* and *oxidisque*, supplies no such nomenclature. The same insurmountable objection applies to the *oxidule* of Fourcroy and Hauy. Besides, Mr. Hassenfratz forgets, that the term *oxidule*, though it does well enough in French, may not be suited to other languages. For instance it would neither be introduced into English nor German, without doing violence to the genius of both languages.

VI. The preceding experiments were made about a year ago; indeed immediately after perusing Mr. Hassenfratz's dissertation. I publish them at present, to put the chemical public on their guard respecting the inaccurate statement of the composition of oxides of iron, which I have introduced into my *System of Chemistry.* I inserted the result of them in the appendix to the 4th edition of that work; but thought it requisite likewise to publish the details, that those who are in possession of preceding editions may be aware of the inaccuracy and correct it.

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